
The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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The American Historical Review

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Cover illustration: A 1797 portrait by Anne-Louis Girodot-Triosin of a deputy to the National Assembly from Saint Domingue, Jean-Baptiste Mars Belley. Born in Africa, he was a former slaveowner himself and came to Paris to speak for general emancipation. He stands beside a bust of Abbé Raynal. © Musées Nationaux (MV 4616). This issue of the *AHR* celebrates the bicentennial of the French Revolution. For a reference to Belley, see the article by David Geggus, "Racial Equality, Slavery, and Colonial Secession during the Constituent Assembly," 1305.

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In This Issue

During the past twelve months, the French Revolution has been celebrated with fireworks and public speeches, scholarly conferences and magisterial new overviews. Whether popular or academic, such events have occasioned lively discussions of the meaning of the revolution. In a special issue of the *AHR*, we continue the debate with five essays first presented at the International Congress on the History of the French Revolution in May of 1989. **Lynn Hunt**, a member of our Board of Editors, helped us to organize this issue, and we much appreciate her generous assistance. Although the articles represent but a small segment of contemporary scholarship on the revolution, their diversity suggests that, even after two hundred years, we have much to learn about the events of 1789.

Sara Maza uses an eighteenth-century court case to demonstrate the ways in which private as well as public issues shaped the political consciousness of late eighteenth-century France. In 1786, the count of Sanois brought suit against his family for having arrested and illegally detained him. The trial briefs subsequently written on behalf of the count by his lawyer attracted much public attention and transformed a story of domestic discord into a political parable. Sanois became a symbol of virtuous republican masculinity and his wife of the corrupt feminized court.

For too long, **Peter McPhee** argues, both Marxists and revisionists have presented a negative picture of the impact of the peasant revolution on the economic development of nineteenth-century France. Data on changing patterns of production suggest that, instead of retarding capitalism, the greater specialization on small holdings after the French Revolution increased the agricultural productivity of nineteenth-century France. In addition, McPhee uses comparative research on agrarian capitalism and the work of Anatolii Ado to challenge the applicability of modernization theory to rural France.

It has long been thought that relations between business and the state became dominated by laissez-faire policies during the French Revolution. **Jean-Paul Hirsch** takes a fresh look at this proposition. He points out that, while notions of laissez-faire came to dominate political discourse, merchants and manufacturers remained unwilling to abandon the advantages of state regulation of trade. They adopted the rhetoric of free enterprise but, in many specific instances, lobbied for the continuation of practices antithetical to it.

Most studies of the French Revolution pay little attention to its impact on colonial affairs, despite the importance to late eighteenth-century France of its overseas empire. In his examination of the evolution of attitudes toward colonial autonomy, race relations, slavery, and the slave trade during the years 1789–1791, **David Geggus** finds that the Constituent Assembly was reluctant to confront problems that so directly challenged the prosperity of the mother country as well as the universalist claims of the revolution.

In the last essay, **Jack Censer** offers a provocative overview of recent writings on the French Revolution. The authors in this newest generation of historians, while diverse, are more negative about the revolution than their predecessors. Censer looks first at the analyses of ideology and discourse by François Furet and Lynn Hunt and then at the new narrative histories produced by William Doyle, D. M. G. Sutherland, Simon Schama, and John Bosher.

Our mail suggests that readers found the issues raised by the exchange between David Harlan and David A. Hollinger in the June 1989 issue of the *AHR* important and thought provoking. In the *Notes and Comment* section, **Joyce Appleby** joins the discussion. She acknowledges the importance of deconstructionist critiques of language but finds validation for contextualist approaches in historians' shared experience of their enterprise.

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Sarah Maza, associate professor of history at Northwestern University, works on the social and cultural history of eighteenth-century France. She is especially interested in questions of "representation," or the interrelationship between social formations and images of society. In 1983, Princeton University Press published her study *Servants and Masters in Eighteenth-Century France*. She is currently writing a book on the great court cases of the French prerevolutionary decades, arguing for the important role played by stories of private scandal in shaping political consciousness in the 1770s and 1780s.

Domestic Melodrama as Political Ideology: The Case of the Comte de Sanois

SARAH MAZA

HOW DID FRENCH MEN AND WOMEN OF THE LATE EIGHTEENTH CENTURY learn to think about politics in a new way? How did at least some of the subjects of a hereditary monarch come to view themselves, in the years before 1789, as citizens of a secularized “nation”? Since the nineteenth century, every generation of scholars has brought new information and hypotheses to the classic question of the ideological origins of the French Revolution. The current generation has tended to favor socially grounded approaches in the tradition pioneered by Daniel Mornet in France and, in the United States, by Robert Darnton. Instead of rereading Montesquieu and Rousseau, they have grubbed about in archives, tracking down information about the production, dissemination, and consumption of printed matter, the contents of readers’ libraries, and the cat-and-mouse games that writers and publishers played with the authorities. As a result, the intellectual and cultural landscape of prerevolutionary France now looks much different, more varied and colorful, than it did twenty years ago.¹

Perhaps inevitably, some scholars have begun to worry that this emphasis on the social context of ideas threatens to go too far, diverting our attention from the texts themselves. Recently, for instance, Keith Baker warned us against the reification of books, texts, and ideas. We do need to know who bought and read what books and pamphlets, Baker argued, but that is not sufficient, since texts acquire meaning only in relation to discursive contexts: “In order to understand the Revolution as a political—that is to say, public—event, we need to reconstitute the field of political discourse in which it occurred.”² Some of the leading scholars in the field have been engaged in precisely the task that Baker

¹ Daniel Mornet’s *Les Origines intellectuelles de la Révolution française* (Paris, 1933), a social history of ideas in eighteenth-century France was the precursor of and model for most of this work. Since the mid-1960s, significant contributions include: Genevieve Bolleme, et al., *Livre et société dans la France du XVIII^e siècle*, 2 vols. (Paris, 1965–70); Daniel Roche, *Le Siècle des lumières en Province: Académies et académiciens provinciaux, 1680–1789*, 2 vols. (Paris, 1978); Robert Darnton, *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775–1800* (Cambridge, Mass., 1979), and *The Literary Underground of the Old Regime* (Cambridge, Mass., 1982); Henri-Jean Martin and Roger Chartier, eds., *Histoire de l’édition française*, vol. 2 (Paris, 1984); Jack Censer and Jeremy Popkin, eds., *Press and Politics in Pre-Revolutionary France* (Berkeley, Calif., 1987); Nina Gelbart, *Feminine and Opposition Journalism in Old Regime France: The Journal des Dames* (Berkeley, Calif., 1988); Robert Darnton and Daniel Roche, eds., *Revolution in Print: The Press in France, 1775–1800* (Berkeley, Calif., 1989).

² Keith Michael Baker, “On the Problem of the Ideological Origins of the French Revolution,” in Dominick LaCapra and Steven Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 206.

advocated, uncovering “a field of political discourse, a set of linguistic patterns and relationships that defined possible actions and utterances and gave them meaning.”³ Dale Van Kley has demonstrated that the radical political rhetoric of the 1780s borrowed many of its concepts and linguistic categories from the religious terminology used earlier in the century by embattled Jansenists; Mona Ozouf has traced in the writings of politicians, administrators, and men of letters the emergence of a concept of “public opinion” that increasingly served, she argued, as a surrogate for divine-right authority; Baker himself has described political thought on the eve of the revolution as coalescing around three different “visions,” the judicial, the administrative, and the political.⁴

This new approach, which one might dub “structural-linguistic,” owes a great deal to the pathbreaking work of intellectual historians like J. G. A. Pocock and Quentin Skinner, who, working on other times and places, have demonstrated what can be gained from a rigorously contextual understanding of the terms and categories employed by political writers.⁵ Applied to the prerevolutionary period, it already has proved fruitful: we are now much closer to understanding how such important concepts as “constitution,” “representation,” “despotism,” and “public opinion” were created or redefined in the waning years of the Old Regime and how that usage came to shape the political culture of the revolution itself.

Timely and innovative as it is, this body of work remains for the most part wedded to a rather conventional assumption, captured in Baker’s automatic characterization of the revolution as “a political—that is to say, public—event.” Structural-linguistic analyses have made us aware of a much greater range of writers, styles, and ideas: muckraking pamphleteers, Jansenist magistrates and lawyers, and even brilliant apologists for the monarchy as well as the canonical philosophes. But the texts studied in order to “reconstitute the field of political discourse” of the prerevolutionary era continue to be those that explicitly and centrally concentrate on politics in the sense of government and public affairs. Other sorts of texts, including novels, plays, treatises about sexuality and private life, and other kinds of sentimental or fictionalized literature, assumed to have had no bearing on the genesis of a new “political discourse” on the eve of the revolution, are left mostly to literary scholars and to historians of *mentalités*. Yet, as every historian of the period knows, Rousseau’s novel of marital love and

³ Baker, “On the Problem of the Ideological Origins of the French Revolution,” 212.

⁴ The best examples of this new approach are collected in a recent volume edited by Keith Baker, *The Political Culture of the Old Regime* (Oxford, 1987); see Dale Van Kley, “The Jansenist Constitutional Legacy in the French Pre-Revolution,” 169–202; Mona Ozouf, “L’Opinion Publique,” 419–34; as well as the articles by Marina Valensise, Bronislaw Baczko, and Baker himself. See also Keith Michael Baker, “French Political Thought at the Accession of Louis XVI,” *Journal of Modern History*, 50 (1978): 279–303; and “Politics and Public Opinion under the Old Regime: Some Reflections,” in Censer and Popkin, *Press and Politics*, 204–46. This new approach, which relies heavily on textual analysis, is explicitly or implicitly influenced by François Furet’s provocative interpretation of the revolution as a linguistic or semiotic event in *Penser la Révolution Française* (Paris, 1978).

⁵ See J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, N.J., 1975); and *Virtue, Commerce, and History: Essays on Political Thought and History, Chiefly in the Eighteenth Century* (Cambridge, 1985); Quentin Skinner, “Hermeneutics and the Role of History,” *New Literary History*, 7 (1975): 209–32; and *The Foundations of Modern Political Thought*, 2 vols. (Cambridge, 1978).

extramarital passion, *La Nouvelle Héloïse*, changed the intellectual as well as emotional lives of many of its readers, while his *Social Contract* went mostly ignored or misunderstood.⁶

In short, we have tended to impose on our understanding of this question of ideological origins a sharp distinction between “public” and “private” matters, which only began to appear in its modern form well after the revolution. As contributors to the recent *Histoire de la vie privée* point out, the most usual antonym for the word *public* in classical French was not *privé* but *particulier*.⁷ Premodern Western societies distinguished not so much between intimate matters and affairs of state as between subjects of general interest and concern and those with “particular” relevance. In such societies, a personal or intimate matter could easily be understood as a “public” issue if its general relevance were made clear.

This article attempts to illustrate these points through the example of a nobleman, the Comte de Sanois, who in 1786 brought suit against his wife and other members of his immediate family. Analysis of the trial briefs written in the Sanois case suggests the limitations of the usual distinctions between fictional and political writing, between emotional persuasion and rational argument, and between the public and the private spheres.

THE CASE OF THE COMTE DE SANOIS provoked a brief but intense flurry of interest among the Parisian public in the summer of 1786. It was but one in a series of increasingly sensational *causes célèbres* in the 1770s and 1780s. Earlier, in 1762, the great Voltaire had taken up the defense of an obscure Protestant widow and her children: their husband and father, Jean Calas, had been broken on the wheel, the victim of religious prejudice and bigotry.⁸ A decade later, a moderately successful playwright named Pierre-Augustin Caron de Beaumarchais had attained celebrity status by publishing the briefs that he wrote for his own defense in a series of court cases brought by a grasping nobleman and tried by a corrupt magistrate.⁹ Voltaire and Beaumarchais had each realized how effectively court cases could serve as vehicles for self-promotion. Their example

⁶ On the *Social Contract*, see Joan McDonald, *Rousseau and the French Revolution, 1762–1791* (London, 1965); Durand Echeverria, “The Prerevolutionary Influence of Rousseau’s *Social Contract*,” *Journal of the History of Ideas*, 32 (1972): 542–60; Roger Barny, *Prélude idéologique à la Révolution Française: Le Rousseauisme avant 1789* (Paris, 1985); on *La Nouvelle Héloïse*, see Mornet, *Origines Intellectuelles*, 95–96, and the preface to his edition of *La Nouvelle Héloïse* (Paris, 1925); Robert Darnton, “Readers Respond to Rousseau: The Fabrication of Romantic Sensitivity,” in his *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1985), 215–56.

⁷ Roger Chartier, ed., *Histoire de la vie privée: De la renaissance aux lumières* (Paris, 1986), 15–18, 23, 413–53.

⁸ Accounts of this case include Marc Chassaigne, *L’Affaire Calas* (Paris, 1929); David Bien, *The Calas Affair: Persecution, Toleration and Heresy in Eighteenth-Century Toulouse* (Princeton, N.J., 1960); and Edna Nixon, *Voltaire and the Calas Case* (New York, 1961).

⁹ Louis de Loménie, *Beaumarchais et son temps: Etudes sur la société en France au XVIII^e siècle*, 2 vols. (Paris, 1856), 1: 231–447; George Havens, *The Age of Ideas: From Reaction to Revolution in Eighteenth-Century France* (New York, 1955), chap. 22; John Hampton, “The Literary Technique of the First Two *Mémoires* against Goetzman,” *Studies on Voltaire and the Eighteenth Century*, 47 (1966): 177–205.

was not lost on the most talented and ambitious of a generation of young lawyers who yearned for the sort of public adulation enjoyed by some men of letters. In the hands of a skillful barrister, they had learned, even a mundane affair like that of Sanois could be handled in such a way as to make it a *cause célèbre*.

In the spring of 1785, Jean-François de la Motte-Geffrard, count of Sanois, plagued by severe financial and domestic problems, had abandoned his family and run off to Switzerland. His wife, daughter, and son-in-law had him arrested there by the French authorities, brought back to France, and imprisoned in the insane asylum of Charenton with a *lettre de cachet*.¹⁰ (*Lettres de cachet* were secret missives emanating from the sovereign and signed by the secretary of state that, bypassing the normal judicial processes, ordered the imprisonment or exile of an individual, often at the behest of members of his or her own family.¹¹) While Sanois struggled for his release from Charenton, which he obtained after nine months of confinement, his wife sued for a separation on the grounds of her husband's cruelty, insanity, and fraudulent bankruptcy and demanded the restitution of 400,000 livres of their joint assets with which, she claimed, he had absconded.

Like many other such sordid family squabbles, the Sanois case became known to the public mainly through the publication of *mémoires judiciaires*, printed versions of the lawyers' defenses of their clients. In Old Regime France, it was standard practice, as codified in the great Criminal Ordinance of 1670, to hold hearings in private.¹² Judges or their delegates examined defendants and witnesses in their chambers, while a clerk recorded the interrogations on paper. The paperwork thus generated became, along with assorted petitions and appeals, part of the file known as the trial "bag" (*sac du procès*); this was reviewed at the end of the process by a panel of judges and served as the basis for the final judgment and sentencing. Within this system, in civil cases, lawyers were allowed to plead orally before the judges, but their contribution was most frequently a written one. On the basis of their client's testimony and other evidence, making use of their legal expertise and rhetorical skills, they drew up one or several briefs or memoranda that became part of the file and were read by the judges before a verdict was handed down.

In theory, these *mémoires*, which could simply be handwritten, were documents internal to the courts. Increasingly, however, and especially in the wake of the Calas affair of 1762, it had become the practice in significant court cases to print up multiple copies of trial briefs for perusal by anyone, friend or foe of the defendant, who took an interest in the case.¹³ Although other documents

¹⁰ See case summary in *Mémoires secrets pour servir à l'histoire de la république des lettres*, 36 vols. (London, 1777–89), 32: 199–200.

¹¹ Marcel Marion, *Dictionnaire des institutions de la France aux XVII^e et XVIII^e siècles* (1923; rpt. edn., Paris, 1984), 329–31. For a fuller discussion, see Frantz Funck-Brentano, *Les Lettres de cachet à Paris* (Paris, 1903), ix–xlv.

¹² The following description is based on Jean Imbert, *Quelques procès criminels des XVII^e et XVIII^e siècles* (Paris, 1964), 2–7; and André Laingui and Arlette Lebigre, *Histoire du droit pénal*, 2 vols. (Paris, 1979), 2: 81–103.

¹³ For an example of the use of printed *mémoires* in a protracted family squabble early in the century, see Maurice Daumas, *L'Affaire d'Esclans: Les conflits familiaux au XVIII^e siècle* (Paris, 1988),

pertaining to cases, such as announcements (*monitoires*) and appeals (*requêtes*), were also sometimes printed up and disseminated, the *mémoires* remained the principal means whereby secret trial proceedings were made public, the main bridge between the courtroom and the street. In addition, since the fiction persisted that these were internal memoranda, they were not subject to official censorship and could be rapidly produced and openly distributed or sold.¹⁴

These factors, in addition to the often gripping, sensational contents of many *mémoires*, account for their development, in the later eighteenth century, into a highly popular form of pamphlet literature. In the 1770s, press runs of three to six thousand were usual in big court cases, and, by the time of the Sanois case, printings of ten thousand or more were not uncommon.¹⁵ Lawyers and other writers increasingly claimed that trial briefs did not just amuse or satisfy a curious public but also fulfilled an important political function. "What is a *mémoire*?" wrote a lawyer named Ambroise Falconnet in 1775. "Here is how I define it. A *mémoire* establishes the rights of a citizen by explaining them to the judges and to the public."¹⁶ Even a writer like Sébastien Mercier, who could be scathingly contemptuous of the bickerings and pretensions of contemporary lawyers, wrote in praise of trial briefs in his *Tableau de Paris*. One of the most useful results of the discovery of printing, he wrote, was that its use in court cases "could serve to interest a whole nation, making it attentive to the rights of the unfortunate who has neither rank nor credit."¹⁷

Sanois, of course, lacked neither rank nor credit, and he was shrewd or wealthy enough to secure the services of a man who turned him into a martyr of sorts and his case into a political crusade. The single most important publication to come out of the Sanois case was the defense of the count published in July of 1786 by the lawyer Pierre Louis de Lacrosette. The *Gazette de Leyde* gave it an enthusiastic review, praising its "order and method," "warmth and energy," and predicting that readers would find it riveting from the very first pages.¹⁸ A few days later, the newsletter *Mémoires secrets* reported that the pamphlet was indeed attracting much attention; in fact, it was rumored that the queen herself had had the trial brief read to her as she recovered from childbirth and that her bumbling husband had exclaimed that he wanted to read it, too, because "they are having me give out *lettres de cachet* right and left and I don't know the first thing about it."¹⁹ If Louis XVI did read the pamphlet, he may not have liked

especially 11–14; for a more general discussion of the function and evolution of these documents, see Sarah Maza, "Le Tribunal de la nation: Les mémoires judiciaires et l'opinion publique à la fin de l'ancien régime," *Annales: Economies, sociétés, civilisations*, 42 (January 1987): 75–90.

¹⁴ Maza, "Le Tribunal de la nation," 75–78; Hans-Jürgen Lüsebrink, "L'Affaire Clereaux (Rouen, 1786–1790): Affrontement idéologiques et tensions institutionnelles autour de la scène judiciaire au XVIII^e siècle," *Studies on Voltaire and the Eighteenth Century*, 191 (1980): 892–900.

¹⁵ Maza, "Le Tribunal de la nation," 77.

¹⁶ "Mémoire pour M. Falconnet, avocat en la cour . . ." (1775), reprinted in Ambroise Falconnet, *Le Barreau français, partie moderne*, 2 vols. (Paris, 1806–08), 1: xvi.

¹⁷ Louis-Sébastien Mercier, *Le Tableau de Paris*, 12 vols. (1781; nouvelle édition, Amsterdam, 1783–88), 6: 319.

¹⁸ *Nouvelles extraordinaires de divers endroits*, known as the *Gazette de Leyde*, July 20, 1786.

¹⁹ *Mémoires secrets*, 32: 199–200, 209–10, 238.

what he found in it. On account of its contents, Lacretelle was denied his two thousand-livre government pension a couple of months later.²⁰

Lacretelle's brief for Sanois effectively captured the attention of readers and reviewers and, no less effectively, alienated the authorities—these two responses often going hand in hand at the time. It did so for reasons typical of the *mémoire judiciaire* as a genre of writing (exploited by Lacretelle with unusual skill) that also cleverly played to contemporary tastes and concerns. First, Lacretelle made full use of the literary, specifically, the melodramatic, potential of his story. Second, that story raised issues of gender and power with special resonance for readers at this historical juncture. Finally, this two hundred-page text, which began with a dramatic account of domestic strife and ended in a diatribe against *lettres de cachet*, offered a powerful coupling of private and public issues.

SANOIS'S LAWYER, PIERRE-LOUIS DE LACRETELLE, was thirty-five years old at the time of the case. He had arrived in Paris eight years earlier from his native Metz, already having made something of a name for himself by publishing *mémoires* noted for their literary merit, and he was immediately admitted to the prestigious Parisian Order of Barristers. Although there is no reason to doubt the sincerity of Lacretelle's liberal and reformist convictions, it also seems clear that this scion of the legal profession (his father and brother were lawyers, too) was a shrewd careerist, a man skilled at making the right moves and getting to know the right people.²¹

As a young man, Lacretelle realized that judicial and penal reform were pressing and popular issues. Accordingly, he began to produce essays such as *On the Causes of Crimes* and *On the Multiplicity of Laws*. In 1784, his *Discours sur le préjugé des peines infamantes* earned him the first prize in a contest set up by the Academy of Metz; the second prize went to another young lawyer of similar talent and inclinations named Maximilien de Robespierre. Lacretelle's taste for involvement in public affairs was to lead him straight into the revolutionary turmoil. The late 1780s found him churning out pamphlets about judicial and governmental reform; by 1791, as a member of the moderate Feuillant party, he was elected one of the deputies of the city of Paris to the Legislative Assembly.²² At the same time, like many barristers of his generation, Lacretelle pursued a literary career and cultivated connections in the world of letters. His friends were up-and-coming young writers like Jean-Baptiste Suard and Dominique-Joseph Garat (with whom he once shared a prize from the Académie Française), and, as his fame increased, he rubbed shoulders with the likes of Jean LeRond d'Alembert, Jean-François Marmontel, the Marquis de Condorcet, and Jean-François de La Harpe and helped edit the *Mercure de France*.

²⁰ *Mémoires secrets*, 33: 66.

²¹ See the articles on Pierre-Louis de Lacretelle in Louis Michaud, *Biographie universelle ancienne et moderne* (Paris, 1877), vol. 22; Ferdinand Hoefer, *Nouvelle biographie universelle* (Paris, 1852–66), vol. 28; and Alphonse Rabbe, *Biographie universelle et portative des contemporains* (Paris, 1854), vol. 3.

²² Lacretelle's prerevolutionary writings include *De la convocation prochaine des Etats-Généraux en France* (n.p., 1788); and *Projet de cahier pour le Tiers-Etat du bailliage et du vicomté de Paris* (n.p., 1789).

Barristers like Lacretelle refined their skills and reputations as men of letters because they were aware that the *mémoires* most likely to meet with resounding success were those that read like works of fiction. In the late eighteenth century, many of the most successful trial briefs were first-person narratives, analogous to novelistic memoirs and allegedly written by the defendants. Readers, however, were well aware of the real authors. One of Lacretelle's early successes, a trial brief written for a young actress accused of poisoning her sister, was a first-person account that studiously imitated the tone of a guileless teenage girl. "Although still very young," it began, "I have heard tell of many evils; but I am subjected to one today that passes anything I could have imagined." And it ended: "What harm can I have done to them to bring such outrages upon myself? But rather, what harm have I ever done to anyone?"²³

When Lacretelle sat down to write his first trial brief for Sanois in 1786, he was very much under the sway of Rousseau's *Confessions*, which had been published posthumously four years earlier. What he produced, even so, was not an autobiographical account but a more complex layering of voices: his own third-person narrative based, he claimed, on memoirs written by Sanois himself. Although the readers were not given access to the count's memoirs, they were repeatedly reminded, in tones reminiscent of Rousseau, that displaying one's life in writing was an automatic proof of virtue. The memoirs that Sanois had written to justify himself were, according to his lawyer, "an exact record of the misfortunes he suffers, of everything that occupies his mind and stirs his soul. Crime has not yet availed itself of this sort of defense, for every line would create impediments, and it could betray itself in a single page; but [such a defense] is perfectly suited to virtue, which has nothing to hide, wants to tell all, and opens up a whole life to illuminate a single fact."²⁴

Rousseauian references occur repeatedly throughout the remainder of the text. It was convenient, of course, that Sanois had fled to Lausanne in search of "morals closer to those of his own heart," since, thanks to Jean-Jacques, the Swiss Alps were the ideal place for locating a person on a high moral ground.²⁵ There were sentences taken almost verbatim from Rousseau ("He tells the good and the bad about his enemies as about himself"²⁶) and several dramatic encounters with the Supreme Being: "Then I will appear before the fearsome tribunal of the Being who Creates and Avenges and will address to him my justification in the sincerity of my heart."²⁷

The *Mémoire pour le Comte de Sanois* might not have created such a stir had it simply sought to establish Sanois's innocence by lacing the account of his life with

²³ "Mémoire pour la Demoiselle D*** l'aînée, actrice de la comédie italienne, pensionnaire du roi" (1779), in Pierre-Louis de Lacretelle, *Oeuvres*, 6 vols. (Paris, 1823–24), 1: 329, 339.

²⁴ Pierre-Louis de Lacretelle, *Mémoire pour le Comte de Sanois, ancien aide-major des gardes françaises . . . contre ses accusateurs* (Paris, 1786), 11.

²⁵ Lacretelle, *Mémoire pour le Comte de Sanois*, 38.

²⁶ Lacretelle, *Mémoire pour le Comte de Sanois*, 11. Compare with Rousseau's celebrated statement "J'ai dit le bien et le mal avec la même franchise," *Les Confessions* (Paris, 1964), 4.

²⁷ Lacretelle, *Mémoire pour le Comte de Sanois*, 73; see also Lacretelle's *Supplément au mémoire du Comte de Sanois* (Paris, 1786), 27.

Rousseauian references. However, in a significant stylistic innovation, Lacretelle produced for his client not just a sentimental biography but a “framed” narrative whose “authenticity” was established by the lawyer as first-person narrator and as reader. In the first pages of the *mémoire*, the reader is introduced not to Sanois but to Lacretelle himself, who, in the manner of a Sherlock Holmes, is sitting in his rooms minding his own business when a haggard-looking stranger bursts in upon him. While the lawyer-detective notes some telltale signs (a face drawn by distress and illness rather than old age, shabby but decent clothes, the ribbon of the Order of Saint-Louis), the man at first refuses to disclose his name, with the comment, “It would not be known to you, or if it were it would be through the calumnies of which I am the victim.”²⁸

There ensues a vivid dialogue between the lawyer and his future client, in the course of which an outline of Sanois’s story emerges and Lacretelle’s initial wariness turns into indignation: “Monsieur,” exclaims the lawyer, “everything about your story amazes me; it is a continual reversal of natural feelings and the rules of common behavior . . . How can the authorities, at last enlightened . . .”—“The authorities, Sir,” replies Sanois, “have the means for arresting citizens everywhere; they have no forms by which to judge them.”²⁹ Sanois eventually leaves the lawyer two fat volumes of his autobiography, which Lacretelle proceeds to read and then to summarize for the benefit of his own readers.

At least one of those readers, Sanois’s son-in-law, the count of Courcy, accused of having aided in the conspiracy against his wife’s father, scathingly dismissed the *mémoire*’s opening gambit. “I am not speaking,” he fumed, “of the dialogue that M. de la Cretelle places at the beginning of his work, in which he seems to suppose that the gradual impression that M. de Sanois’s account makes on him will be an argument or an object of interest to his readers. It seems to me that, in this respect, there is no lawyer who cannot in the same way substitute persuasion for reason; and if I were examining M. de la Cretelle’s *mémoire* as a critic, I would add that this new form of dialoguing with one’s client is merely insipid and bizarre.”³⁰ But less partisan reviewers, like one in the *Gazette de Leyde*, praised the opening of the brief as artful and dramatic, worthy of engaging the attention of readers.³¹

In complaining that the lawyer had substituted “persuasion for reason,” Courcy was going against the grain of his time, since emotional persuasion was precisely what lawyers attempted to provide, and readers sought, in a good *mémoire*. “If it excites, according to the needs of the case, pity, indignation, contempt, horror, and always conviction,” Falconnet wrote, “the *mémoire* is good.

²⁸ Lacretelle, *Mémoire pour le Comte de Sanois*, 4–5.

²⁹ Lacretelle, *Mémoire pour le Comte de Sanois*, 8.

³⁰ Comte de Courcy, *Réponse du Comte de Courcy à M. le Comte de Sanois et à M. de la Cretelle son défenseur* (Paris, n.d.), 10.

³¹ The excerpt from the *Gazette de Leyde* is reprinted in the “Extrait des Gazettes diverses qui ont fait mention de mon affaire,” published as an appendix to *Compte des recettes et dépenses de l’administration du Comte de Sanois* (Geneva, 1787), 94.

It is excellent if it carries these different sensations to the highest degree.”³² The recourse to sensation as a basis for knowledge and judgment derived from Lockean epistemology; standard handbooks for lawyers, like that written in 1767 by Pierre-Louis Gin, took an explicitly Lockean line, arguing that sensation was the primary form of knowledge: “It is sensations that etch our first ideas in us, reflection that multiplies, compares, and judges them.”³³

Playing on the reader’s emotions or “sensations” was therefore considered no less legitimate a form of argument than purely cerebral reasoning; contemporary lawyers routinely adorned their briefs with descriptions, dialogues, conversations, and confrontations. Lacretelle, however, went one step further: casting himself in the role of impartial reader rather than writer, he heightened the effect of his account by anticipating and echoing his readers’ reactions: “I cannot give a better idea of the case I am about to defend,” he wrote, or “prepare for the impression it must create, than by retracing the ideas, the impressions, which I received myself.”³⁴ Never had Lockean sensationalism been better pressed into the service of forensic argument. The story thus set up to arouse the emotions of readers was predictably one of clear moral dichotomies, of complete innocence hounded by absolute evil in the persons of Sanois’s “unnatural” wife, daughter, and son-in-law, and the officials whom they persuaded to carry out their wishes. Sanois’s life, as told to the reader, is one of continual suffering, and the contrast between the count’s probity and the miseries he endures is stressed throughout the text in tones of unrelenting hyperbole. For instance, when Sanois is released from Charenton, he immediately makes his way to the house of his one friend, a man of exemplary kindness and loyalty. “What will be his pleasure when he sees me again! With what zeal he will take up my vindication! Great God, I bless you in my very misery! you left me at least one faithful friend. Alas! He was no more.” Sanois finds only “a desolate widow, weeping for the best of men. O, what desolation is life!” The widow in question proves to be, as one might expect, a superlatively generous and compassionate soul. Under his lawyer’s pen, Sanois’s life becomes one framed by moral polarities, by “I know not what touching mixture of virtue and crime.”³⁵

WATCHING THE COUNT SUCKED INTO THE VORTEX of betrayal and persecution set in motion by his evil relatives, “one is equally struck,” wrote Lacretelle, “by the excess of innocence and the excess of calumny.”³⁶ Lacretelle’s narrative of the count’s life is not so much sentimental as melodramatic, a pure example of the genre Peter Brooks has called the “mode of excess.”³⁷ Melodramatic writing, with its hyperbolic language, strong emotions, and moral polarization, first

³² Falconnet, “Mémoire pour M. Falconnet,” xxi.

³³ Pierre-Louis Gin, *De l'Eloquence du barreau* (Paris, 1767), 200–01.

³⁴ Gin, *De l'Eloquence du barreau*, 3.

³⁵ Lacretelle, *Mémoire pour le Comte de Sanois*, 101.

³⁶ Lacretelle, *Mémoire pour le Comte de Sanois*, 104.

³⁷ Peter Brooks, *The Melodramatic Imagination: Balzac, Henry James, Melodrama, and the Mode of Excess* (1976; rpt. edn., New York, 1985).

appeared in France in the new dramatic form pioneered in the 1750s and 1760s by writers like Denis Diderot, Pierre-Augustin Caron de Beaumarchais, and Louis-Sébastien Mercier and known as the *drame* or *genre sérieux*.

In his manifesto for the new genre, the *Entretiens sur le Fils Naturel*, Diderot claimed that the essence of the *genre sérieux* was its conveying of a moral lesson that would impress on spectators “the love of virtue and the horror of vice.” Diderot’s premise, no doubt derived from his Lockean beliefs, was that moral truths were not inscribed in the unchanging reality of a person’s “character,” still less in the blind workings of “destiny,” but rather grew out of the dynamic relationships between different groups in society or between the members of a family.³⁸ There can be little doubt that the authors of *mémoires* found inspiration for their accounts of social or familial discord in the declamatory and moralizing portrayals of contemporary life that were the standard fare of the *drame*.³⁹

If trial briefs borrowed from the new “bourgeois” drama its tears and tirades, dramatists, in turn, began to consider the courtroom an important source of inspiration for their plays. The playwright Mercier suggested in his treatise *Du Théâtre* (1773) that great judicial cases be replayed on stage and that spectators confirm the verdict of the law by cheering the resolution of the case.⁴⁰ Mercier had begun to take an interest in *causes célèbres* as early as 1764, when, visiting Toulouse, he composed a dramatic poem entitled *Calas sur l’échafaud à ses juges* (Calas on the Scaffold to his Judges) “on the very square where the old man was broken on the wheel.”⁴¹ Mercier himself never wrote a play on the subject of the Calas case, but several other writers did, although none of these were actually performed prior to the revolution.⁴²

Conversely, Lacretelle himself took an active interest in the new “bourgeois” or “serious” genre. His collected works contain fragments such as “On the Genre Called *drame*”; he also composed, after the revolution, a melodrama set in the Old Regime, which combined political themes with familial ones, and for which he pointedly borrowed the title of Diderot’s most famous *drame*, “Le Fils Naturel.”⁴³ In his notes on the play, published along with it in 1802, Lacretelle argued for the intermingling of the *tragédie bourgeoise* with the *tragédie politique* and pointed to the courtroom as a source of inspiration for the former: “It has not yet been said enough: the annals of the law courts and the anecdotes now

³⁸ Denis Diderot, “Entretiens sur Le Fils Naturel,” in *Oeuvres de Diderot*, ed. Andre Billy (Paris, 1951), 126, 125.

³⁹ See Felix Gaiffe, *Le Drame en France au XVIII^e siècle* (Paris, 1910); Michel Lioure, *Le Drame* (Paris, 1963); W. D. Howarth, “The Playwright as Preacher: Didacticism and Melodrama in the French Theatre of the Enlightenment,” *Forum for Modern Language Studies*, 14 (April 1978): 97–115.

⁴⁰ Louis-Sébastien Mercier, *Du Théâtre ou nouvel essai sur l’art dramatique* (1773; rpt. edn., Geneva, 1970), 153.

⁴¹ W. D. Howarth, “Tragedy into Melodrama: The Fortunes of the Calas Affair on the Stage,” *Studies on Voltaire and the Eighteenth Century*, 174 (1978): 123.

⁴² Howard, “Tragedy into Melodrama,” 126–50. Before 1789, two plays about the Calas affair were written in French, three in Dutch, and one in German; it seems that only the German play was performed, in Berlin. In the early 1790s, however, five new plays on the subject, one of them by André Chénier, were written, performed, and published, and the story of Jean Calas’s ordeal, death, and rehabilitation continued to inspire playwrights into the 1830s.

⁴³ “Le Fils Naturel,” in Lacretelle, *Oeuvres*, 4: 6–225; “Sur le genre appelé Drame,” in *ibid.*, 4: 184–88.

gathered in so many private trial briefs are for [the bourgeois tragedy] what history is for the historical tragedy.”⁴⁴

Peter Brooks has argued that the melodramatic impulse is one that seeks to heighten and make explicit the moral meaning of daily life, to bring to light “the cosmic moral sense of everyday gestures.” He connects the birth of melodramatic writing in the mid-eighteenth century, and its triumph in the boulevard theater of the early nineteenth, with the collapse of older transcendent authorities and the sudden opening up of the political sphere. Whether its overt contents are revolutionary or conservative, he writes, melodrama “is in all cases radically democratic, striving to make its representations clear and legible to everyone.” The melodramatic mode represents, in short, the urge to “resacralize” the world, and to do so in terms that are both intensely personal and universally intelligible.⁴⁵ One could argue, then, that the political message of Lacretelle’s *mémoire* was partially inscribed in its very style, in its combination of Lockean subjectivity and “democratically” didactic moralism.

The story couched in these Manichean terms was a familiar tale of domestic discord. At the age of forty, Sanois had consented to a proper, arranged marriage to a woman who, in Lacretelle’s account at least, turned out to be an uneducated and shrewish social climber: “dissipated, prodigal, incapable of being happy in her own house, or of being sought after in other people’s, she was obliged to give herself over to an existence made up of the most senseless agitation.” Madame de Sanois’s frantic quest for social recognition forced her husband into the background of their social lives to the point that guests who came to dinner had no idea he was the master of the house.⁴⁶ Sanois’s hopes for finding consolation in the joys of fatherhood were dashed when his only child, a daughter, grew up to be just as rude and snobbish as her mother, with whom she joined forces. (Naturally, Madame de Sanois’s version of the story was very different, and Lacretelle underplayed the financial reasons that had prompted the count to flee to Switzerland.⁴⁷)

Lacretelle summed up the chaos of the Sanois household as “despotism of the wife and servitude of the husband”; he commented at some length on the notion that the usurpation of domestic power by a wife amounted to a perversion of the most fundamental laws of nature and society.⁴⁸ Of course, there was nothing particularly new about the suggestion that a “woman on top” was emblematic of social disorder⁴⁹; but Madame de Sanois’s misrule of her husband took on immediate political connotations when she resorted to the use of the *lettre de cachet*, which itself had recently become a central symbol of arbitrary authority.⁵⁰

⁴⁴ “Sur le système dramatique du ‘Fils Naturel’” (1802), in Lacretelle, *Oeuvres*, 4: 137.

⁴⁵ Brooks, *Melodramatic Imagination*, 13–14, 15, 16.

⁴⁶ Brooks, *Melodramatic Imagination*, 30–31.

⁴⁷ See Tronson du Coudray, *Mémoire pour la Comtesse de Sanois demanderesse en séparation de biens* (Paris, 1786).

⁴⁸ Lacretelle, *Mémoire pour le Comte de Sanois*, 114–15.

⁴⁹ See Natalie Zemon Davis, “Women on Top,” in her *Society and Culture in Early Modern France* (Stanford, Calif., 1975), 124–51.

⁵⁰ Funck-Brentano, *Les Lettres de cachet*, xli–xlvi; Arlette Farge and Michel Foucault, *Le Désordre des familles: Lettres de cachet des archives de la Bastille au XVIII^e siècle* (Paris, 1982), 357–63.

In 1782, the young count of Mirabeau had enjoyed one of his many *succès de scandale* with the publication of his treatise *Des lettres de cachet et des prisons d'état*, and the following year the equally notorious journalist and pamphleteer Nicolas Linguet had given the public an even more popular account of his secret arrest and imprisonment in the Bastille.⁵¹ In contemporary thinking, *lettres de cachet* and *mémoires* represented two opposite poles of authority and justice: on the one hand, a secret and arbitrary procedure, on the other, an open, public appeal to reason and law.⁵²

The association between “bad” femininity and corrupt despotism was also one of the commonplaces of polemical literature in the 1780s. It is a theme that pervades the abundant underground literature that retroactively attacked the government of Louis XV and of his triumvirate of dictatorial ministers, Maupeou, Terray, and d'Aiguillon. One of the most popular items in this repertoire was a hefty volume entitled *Les Fastes de Louis XV*, an anonymous work published in 1782 that chronicled the political decay of the former reign by focusing mainly on the meteoric rise to power of Madame de Pompadour and then Madame du Barry. The latter especially was said to have seduced the king into a “private, slothful, and voluptuous life” and similarly emasculated his triumvirate of ministers, who were both despots and “slaves crawling at the feet of a prostitute who had ascended in one leap from the brothel to the throne.”⁵³ When Lacretelle had Sanois declare, in a later *mémoire*, that his wife and daughter had used “credit, favors, and everything that human passion can dispense in order to silence the Law and cause my ruin,” readers would have recognized this as a familiar antithesis between the corrupt feminized behavior of the court and the healthy masculinity of the courtroom.⁵⁴

Sanois's repeated invocation of “the Law” as a bulwark against his wife's actions should be understood in connection with the emergence of a symbolic order in which femininity was construed as utterly antithetical to a new definition of the polity grounded in reason and law. What Joan Landes has described as the exclusion of women from the public sphere, before and during the French Revolution, must not be viewed as yet another manifestation of timeless patriarchal sexism. Rather, Landes cogently argued, it was symptomatic of the transition from one social and representational system to another, from the “iconic spectacularity” of the Old Regime—the theatrical, highly feminized worlds of the court and aristocracy—to the “textual and legal order” of the homogeneously male public sphere of the revolution.⁵⁵

⁵¹ Gabriel-Honoré de Riqueti, count of Mirabeau, *Des Lettres de cachet et des prisons d'état* (1782), in Mirabeau, *Oeuvres*, 8 vols. (Paris, 1834–35), vol. 7; Simon-Nicolas-Henri Linguet *Mémoires sur la Bastille* (London, 1783); see Darline Gay-Levy, *The Ideas and Careers of Simon-Nicolas-Henri Linguet: A Study in Eighteenth-Century French Politics* (Urbana, Ill., 1980), 207–09.

⁵² I wish to thank Roger Chartier for this insight.

⁵³ *Les Fastes de Louis XV, de ses ministres, généraux, et autres notables personnages de son règne* (“A Villefranche, chez la Veuve Liberté,” 1782), lvi–lviii; on the popularity of this volume, see Darnton, *Literary Underground of the Old Regime*, 139, 146–47.

⁵⁴ Lacretelle, *Supplément*, 46.

⁵⁵ Joan Landes, *Women and the Public Sphere in the Age of the French Revolution* (Ithaca, N.Y., 1988), chaps. 1–3.

Women, then, were seen as a threat to the healthy “new” politics espoused by liberals such as Lacretelle, especially when the likes of Madame du Barry or Madame de Sanois had access to *lettres de cachet* and other tools of “despotism.” But why? In the late eighteenth century, hostility to the political activity of women seemed to have its ideological origins in the popularity of contractual theories of government and society, especially among members of the legal profession. Popular handbooks for lawyers like those of Antoine Camus and Antoine Boucher d’Argis recommended to their readers, as basic works of jurisprudence, the natural-law classics of Hugo Grotius and Samuel Pufendorf, as well as popularizations and translations of these works by Jean Barbeyrac and Jean-Jacques Burlamaqui.⁵⁶

Lacretelle must have known these works well, and he probably also had some acquaintance with the ideas of Thomas Hobbes, John Locke, and Jean-Jacques Rousseau, since in 1779 he published a long essay called “The Elementary Principles of Covenants,” a summary and discussion of classic contractual theory. In the tradition of this literature, he included a discussion of marriage as a secular contract rather than a sacrament, and his approach seems at first to grant women equal access to the act of covenanting: “Marriage,” he wrote, “is a sort of sharing of oneself. Each spouse gives himself to the other; and each does not give himself without a return, without conditions that can be assimilated to a price.” A few pages later, however, Lacretelle gives a list of persons incapable of covenanting for want of sufficient reason or independence, the usual roster of madmen, imbeciles, children, and “women under the power of their husbands.”⁵⁷

Lacretelle’s discussion of marriage and women in this context illustrates Carole Pateman’s argument with regard to classical contractual theory: that the social contract, the free covenanting of rational beings that gives birth to civil society, nearly always implies a prior sexual contract in the form of marriage, which, by institutionalizing the “natural” unfreedom of women, deprives them of access to the political sphere. In classic contractual theory, she wrote, “entry into the original contract is a rational act”; but, since nearly all of the classic theorists believed that sexual difference implied a difference in rationality, only men were viewed as possessing the attributes necessary for partaking of the original socio-political contract.⁵⁸ For someone like Lacretelle, and for many of his readers, the scandal of Sanois’s story ultimately lay in its revelation that a married woman, unreasonable by nature and unfree by virtue of her prior subjection to her husband, could have access to public, coercive power.

The political message of Lacretelle’s trial brief was thus inscribed in the socio-sexual dynamics of the Sanois couple. But, lest anyone should miss the point, he also stated it directly. In the last third of his *mémoire*, Lacretelle moves

⁵⁶ Antoine Camus, *Lettres sur la profession d’avocat* (Paris, 1772), 5–8; Antoine Gaspard Boucher d’Argis, *Règles pour former un bon avocat tirées des plus célèbres auteurs anciens et modernes* (1711; rev. edn. by Biarnoy de Merville, Paris, 1778).

⁵⁷ “Principes élémentaires des conventions,” in Lacretelle, *Oeuvres*, 1: 347–427, quotations on 358–59, 370–72.

⁵⁸ Carole Pateman, *The Sexual Contract* (Stanford, Calif., 1988), 4–6.

into a general discussion of the history and uses of *lettres de cachet*, culminating in a profession of faith in legal and contractual government. While the use of *lettres de cachet* originated in Louis XIV's forcible pacification of the realm, such a practice flew in the face of the "true principles" of civil society in general and of the French "constitution" in particular. Since society was based on the combination of wills and interests whence emerged the law, every punishment of offenders "must be written into Law and inflicted by the Law." Any exception to this principle was an implicit attack on society. To those who might object on the grounds of monarchical prerogative, Lacretelle answered that, while monarchies might have their origins in usurpation, their survival (unlike that of despotic governments) was implicitly based on popular consent: "It is base flattery to have [monarchy] descend from heaven," he wrote, "it has a truer and equally noble sanction in the consent of the people."⁵⁹

None of this was original, and Lacretelle did cite his source, that manifesto of extreme *parlementaire* constitutional thought, Claude Mey and Nicolas Maultrot's *Maximes du droit public français*. This long and erudite work, first published in 1772, was one of the most important and influential pieces of political writing to come out of the political turmoil of the early 1770s, when Chancellor Maupeou shocked most politically conscious French people by forcibly disbanding the parlements. Basing their arguments on contractual theories, the authors, who were barristers attached to the parlement, argued not only for the parlement's immemorial right to represent the nation's wishes but also, and more radically, for the sovereignty of the nation itself over its monarchical rulers; and they did so while denouncing, in the strongest of terms, ministerial and royal "despotism."⁶⁰ Their treatise naturally included a long section on *lettres de cachet*, since this was one of the most egregious manifestations of "despotism." It concluded with the incendiary statement that "the Prince must therefore never forget that the Throne and the Law have the same source; that the government has no other object, no other end, than to preserve for men the advantages they sought to secure by making themselves subjects and citizens."⁶¹ At the end of his *mémoire*, Lacretelle had thus reworked a long and arid chunk of Mey and Maultrot's *Maximes*, one of the period's most famous manifestos of oppositional politics, into a private, or "particular," story of marital betrayal.

Lacretelle was perfectly open about the fact that he was using the Sanois case to make a political statement. "Any particular affair that leads to general considerations," he wrote, "that is suited to becoming a great object for public attention, must be considered as a great event."⁶² Supporters praised him for

⁵⁹ Lacretelle, *Mémoire pour le Comte de Sanois*, 144 and following, 155, 164.

⁶⁰ Dale Van Kley, "The Jansenist Constitutional Legacy," in Baker, *Political Culture of the Old Regime*, 188–94; Durand Echeverria, *The Maupeou Revolution: A Study in the History of Libertarianism* (Baton Rouge, La., 1985), chap. 2. One of the entries in the journal of the bookseller Hardy suggests that the publication of the *Maximes* in 1772 was perceived as an important event, and that contemporaries at first were struck mainly by its "antidespotic" message; see Siméon-Prosper Hardy, "Mes Loisirs," Bibliothèque Nationale, Paris, Ms. Fr. 6681, entry for August 17, 1772.

⁶¹ Claude Mey and Nicolas Maultrot, *Maximes du droit public français*, 2d edn. (Amsterdam, 1775), vol. 1, section 2, quote on p. 210.

⁶² Lacretelle, *Mémoire pour le Comte de Sanois*, 15.

using the case to “soar above the elements of natural law and the constitutive laws of different governments.”⁶³ Enemies accused him of exploiting his client's troubles to expound his own political views. His son-in-law de Courcy once again was livid and once again missed his mark: “Oh, the love of the public good!” he ironized, “M. de la Cretelle . . . did not want this occasion to pass him by, and he preferred to seize it, as one might expect, rather than to refrain from writing and work at procuring the happiness of an entire family . . . [A]s I spoke to the lawyer, it was the writer who answered me. How could we ever agree?”⁶⁴ But it was precisely Lacretelle's determination to write as a philosophe and not as a mere lawyer, or rather to bridge the gap between the specifics of a particular legal case and the grand principles of ethical and political reasoning, that accounted, in the end, for the success of his *mémoire*.

ONE MIGHT BE TEMPTED to end either with the specific conclusion that Lacretelle had cannily seized on the Sanois affair to further his own reputation and career or with an observation that legal and political abstractions are most effectively publicized when they can be expressed in concrete and personal terms. But the fact that so many of the *causes célèbres* of the 1770s and 1780s revolved around similar conflations of private stories with public issues prompts me to seek a more general, and yet historically specific, interpretation of this pattern.⁶⁵

The theoretical argument that seems the most suggestive in this regard is that put forward by Jürgen Habermas in his famous essay on the birth of the public sphere.⁶⁶ Although Habermas's broad, socioeconomic analysis is a classic Marxist one, his argument about the changing nature of public culture in the eighteenth century departs considerably from Marxist orthodoxy. The first competing “public sphere” to emerge in the eighteenth century as distinct from the state, he argued, was that made up not of the bourgeoisie in general but of the educated reading public. The “bourgeois public sphere” took shape as a self-conscious entity in salons and cafés; it was eventually to challenge established political institutions by appealing to such concepts as “reason” and “law.” But those individuals and groups making up this new “public” were by definition “private” persons, since they had no access to the public culture of the state; their identity was shaped by their experiences within the “intimate sphere” of the nascent bourgeois family. Hence “public opinion” originally appeared among such groups as “a literary blueprint of what would later become a politically oriented public sphere.”⁶⁷ Fictions about intimate experience such as the Diderotian

⁶³ *Lettre d'un avocat à M. de la Cretelle* (n.p., n.d.), 2.

⁶⁴ *Réponse du Comte de Courcy*, 12–13.

⁶⁵ One of the most widely publicized of these cases was the so-called Kornmann affair, a story of “adultery and seduction” that was exploited for political purposes by radical writers like Nicolas Bergasse; tens of thousands of *mémoires* relating to this case circulated in the two years before the outbreak of the revolution. See Robert Darnton, “Trends in Radical Propaganda on the Eve of the French Revolution (1782–1788)” (D.Phil., Oxford University, 1964), chap. 8.

⁶⁶ Jürgen Habermas, *Strukturwandel der Öffentlichkeit* (1962), French translation by Marc de Launay as *L'Espace Public* (Paris, 1986), chaps. 1–2.

⁶⁷ Habermas, *L'Espace Public*, 67, 40.

drame, Lockean and post-Lockean concerns about sensation and psychology, could be reread as expressing the concerns of an emerging “public” that had not yet found its formal political voice. This argument could in turn be usefully connected to Peter Brooks’s insight that melodrama in its heyday was symptomatic both of the urge to “resacralize” the world and of “the impossibility of conceiving sacralization other than in personal terms.”⁶⁸

None of this is meant to suggest, of course, that Habermas’s broad theoretical framework should displace the historical interpretation grounded in specialized research. We need to be sensitive to the nuances of texts and to the complex interaction of different groups (court politicians, magistrates, lawyers, *salonnières*, and men of letters) that shaped the public culture of the late eighteenth century. But the questions raised by a theorist such as Habermas, as well as the insights of literary critics and feminist scholars, must be reckoned with if we are to understand how and why issues of love and authority within the family could prove so powerful in the shaping of public consciousness. By doing so, we will come closer to understanding the peculiar intertwining of private and public concerns that led the French nation to the threshold of a new cultural world.

⁶⁸ Brooks, *Melodramatic Imagination*, 16.

The French Revolution, Peasants, and Capitalism

PETER MCPHEE

FOR THE HISTORIAN DAZED BY THE HEAT AND DUST produced by acrimonious exchanges over the causes, nature, and significance of the French Revolution, the consensus on the meaning of the revolution in the countryside offers a welcome scholarly oasis. There, most agree, the revolution thwarted a fuller liberation of capitalist impulses and retarded the growth of capitalism for most of the nineteenth century. From a Marxist perspective, this juxtaposition of an anti-capitalist, communitarian peasant triumph and a bourgeois and individualistic revolution has always been awkward: was the peasant revolution within, alongside, or contradictory to the dominant bourgeois remaking of France? However, it has also served to highlight what was historically specific about the French Revolution. When Alfred Cobban inveighed against what he labeled—and caricatured as—the orthodox Marxist interpretation in *The Myth of the French Revolution* (1955) and *The Social Interpretation of the French Revolution* (1964), critical reactions from Albert Soboul, Georges Lefebvre, and others hinged on his definitions, his selective use of evidence, and his minimization of the anti-feudal, bourgeois, and transforming nature of the revolution. His characterization of small-holders and the semi-landless as “conservative” and “anti-capitalist” after 1789 was not at issue.¹ Similarly, Georges Lefebvre’s argument of sixty years ago is still accepted by historians of widely divergent schools. According to Lefebvre, although much of the revolutionary legislation pertaining to the economy was intended to facilitate agrarian capitalism, the sheer weight of peasant resistance to agrarian individualism prevented its implementation.² Only recently have historians, and almost exclusively Marxist historians, begun to reexamine the general nature of the peasant movement and

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¹ See, for example, Georges Lefebvre, “Le mythe de la Révolution française,” *Annales historiques de la Révolution française*, 28 (1956): 337–45; Albert Soboul, “Historiographie révolutionnaire classique et tentatives révisionnistes,” *La Pensée*, 177 (1974): 40–58; Alfred Cobban, *The Myth of the French Revolution* (London, 1955); and *The Social Interpretation of the French Revolution* (Cambridge, 1964), chaps. 2, 13.

² Georges Lefebvre, “La place de la Révolution dans l’histoire agraire de la France,” *Annales d’histoire économique et sociale*, 1 (1929): 506–23.

the impact of the revolution on rural life.³ Yet these issues are at the heart of our historical understanding of the French Revolution. Was it a “classic bourgeois revolution,” as Albert Soboul described it in 1973,⁴ or *une voie spécifique*, a unique case of the transformation from feudalism to capitalism? A resolution of the debate needs to combine economic history, contemporary analyses of agrarian “development” in non-Western societies, and theoretical reconsideration of the rural face of the revolution.

ON THE ONE HAND, the effectiveness of the revolution in clearing the terrain for agricultural capitalism is evident from the consistency with which revolutionary legislators emphasized individual property rights, even if, in so doing, they did not always make explicit the advantages of concentration and enclosure. This emphasis is a constant in legislation from the August 1789 decrees on feudalism through the 1791 Rural Code to Jacobin decrees on land and beyond. A view of land as a privately owned commodity and factor of production replaced a feudal, or at least precapitalist, conception of the soil and those who worked, owned, and protected it as linked by obligations and customary practices. Relieved of seigneurial and ecclesiastical exactions and constraints, such land was to be worked by “free” labor. While details of land sales of church and *émigré* property are fragmentary, it is evident that these sales disproportionately extended ownership among those who already possessed considerable property. For example, in the department of the Nord, where 25 percent of all land was sold in the years 1791–1799, the small minority of the population that may be described as urban and rural bourgeois bought up 48 percent of the total land sold.⁵ Market-oriented agriculture was further encouraged by legislation creating a national market: a national and uniform system of law, currency, weights and measures, and the abolition of impediments to internal free trade. In the words of Karl Marx, the revolution represented “the victory of bourgeois ownership over feudal ownership, of nationality over provincialism, of competition over the guild, of the division of land over primogeniture, of the rule of the landowner over the domination of the owner by the land, of enlightenment over superstition, of the family over the family name, of industry over heroic idleness, of bourgeois law over medieval privileges.”⁶

On the other hand, the survival and extension of small holdings was guaran-

³ There are, however, many fine non-Marxist studies of specific communities and regions during the *ancien régime* and revolution, but they have yet to be incorporated into overall syntheses of the revolution. Among such studies in English are Jonathan Dewald, *Pont-St.-Pierre, 1398–1789: Lordship, Community and Capitalism in Early Modern France* (Berkeley, Calif., 1987); Hilton Root, *Peasants and King in Burgundy: Agrarian Foundations of French Absolutism* (Berkeley, 1987); and Patrice Higonnet, *Pont-de-Montvert: Social Structure and Politics in a French Village, 1700–1914* (Cambridge, Mass., 1971).

⁴ Albert Soboul, “Introduction” (1973), in *The French Revolution 1787–1799: From the Storming of the Bastille to Napoleon*, Alan Forrest and Colin Jones, trans. (1962; 1st Amer. edn., New York, 1975), 19.

⁵ Georges Lefebvre, *Les paysans du Nord pendant la Révolution française* (Bari, 1959), 431–525.

⁶ *Neue Rheinische Zeitung*, December 15, 1848, in Karl Marx and Friedrich Engels, *Collected Works*, 50 vols. (London, 1975–), 8: 161.

teed, largely by the political weight of small-holding peasants, part-peasants (small-holders who also did wage work), and laborers but also by the Jacobin vision of a virtuous, independent citizenry.⁷ The sales of *émigré* lands in particular allowed poorer peasants to increase or establish ownership, at very favorable rates of repayment. Lefebvre found that in the department of the Nord, for example, approximately one-third of the 30,000 peasants who acquired land during the revolution had previously been landless⁸; and, in many other areas, land bought by bourgeois and even nobles in 1791–1792 eventually found its way into the hands of small-holders. The law of June 10, 1793, gave local communities the authority to decide whether or not to divide communal lands and required any divisions to be on a per capita basis. While this law was later repealed (on May 21, 1797/2 Prairial V), communal lands were so important to the rural poor that revolutionary and nineteenth-century governments did not interfere with community control over them.⁹ The Napoleonic Code guaranteed the continuation of revolutionary legislation beginning in 1791 that gave all children the rights of succession, a move that facilitated the proliferation of small holdings through *morcellement*. Nor was there any effective policy on regrouping scattered plots into larger holdings (*remembrement*).

While population censuses and agricultural surveys were not completely reliable until well into the nineteenth century, they make clear that, in general terms, the hundred years from 1780 to 1880 saw an absolute increase in the number of small holdings and the area they covered. While the area covered by peasant holdings before the revolution varied between 20 and 50 percent according to region, Lefebvre offered one-third as a national average.¹⁰ The 1884 inquiry by the tax administration revealed that the 13.7 million properties under 20 hectares covered almost half (48.78 percent) of the agricultural land of France. Such an increase in small and medium-sized landholding was accompanied by a decline in the average size of these holdings. For France as a whole, leaving aside plots smaller than 1 hectare, the average size of holdings declined from 12.5 hectares in 1862 to 11 hectares in 1892.¹¹

For these reasons, the “retarding” effects of the revolution on agrarian and hence industrial capitalism have seemed readily apparent to historians of otherwise conflicting schools. Only minor differences exist between Lefebvre’s conclusion in 1933 that small peasants were profoundly attached “to a precapitalist social and economic mode . . . , their influence was conservative much more than revolutionary: they destroyed the feudal regime—but they consolidated the agrarian structure of France,” and Cobban’s thirty years later: “the

⁷ Melvin Edelstein, *La feuille Villageoise: Communication et modernisation dans les régions rurales pendant la Révolution* (Paris, 1977), 317–21.

⁸ Lefebvre, *Les paysans du Nord*, 519–21.

⁹ Albert Soboul, *Problèmes paysans de la Révolution 1789–1848: Etudes d’histoire révolutionnaire* (Paris, 1976), 206–12; Cobban, *Social Interpretation*, 112–15.

¹⁰ Georges Lefebvre, *Etudes sur la Révolution française* (Paris, 1954), 250; see, too, Ernest Labrousse, “The Evolution of Peasant Society in France from the Eighteenth Century to the Present,” in *French Society and Culture since the Old Régime*, Evelyn M. Acomb and Marvin L. Brown, Jr., eds. (New York, 1966), 51.

¹¹ Jean Pautard, *Les disparités régionales dans la croissance de l’agriculture française* (Paris, 1965).

revolution was a triumph for conservative, propertied land-owning classes, large and small. This was one of the factors . . . contributing to the economic backwardness of France in the following century."¹² And Soboul's 1962 position—that "because in France the maintenance of collective practices was left to the wishes of the peasants, the parcelling out of property and the continued subdivision of farming units provided a considerable check on the capitalist transformation of agriculture"—was repeated almost word for word in François Furet's and Denis Richet's 1965 overview of the revolution.¹³ Only Lefebvre's argument that the economic costs were offset by the humanitarian benefits of a slow transition¹⁴ distinguished him from colleagues elsewhere, who continued to assume that the "disappearance" of the peasantry was a necessary precondition of greater agricultural productivity.¹⁵

In short, then, both Marxist and "revisionist" analyses of the rural face of the revolution have emphasized the ways in which the limited but significant victories of the small-holding peasantry restrained capitalist economic development in the nineteenth century. While Marxist historians have been far more concerned than their critics with the nature of the peasant revolution because of their interest in the revolution as a time of wide-ranging social transformation, they, too, have long understood it as a major qualification to this "classic bourgeois revolution."

IT IS ALL THE MORE REMARKABLE THAT, since the mid-1960s and especially since 1973, such a broadly shared notion of the place of the revolution in the history of rural France—indeed, of the revolution itself—has begun to be challenged. The resulting critical reformulation has been almost solely the work of Marxists, in particular, of those involved in the late Albert Soboul's doctoral seminar at the Sorbonne, such as Florence Gauthier, Guy Ikni, and Claude Gindin.¹⁶ Yet almost no trace of their concerns appears in "revisionist" discussions of the revolutionary period, which continue to confine themselves either to the nature of bourgeois-noble relations or to denying the relevance of the revolution to daily life.¹⁷ In William Doyle's *Origins of the French Revolution*, the peasantry

¹² Lefebvre, *Etudes sur la Révolution française*, 250, 257; Cobban, *Social Interpretation*, 169–70.

¹³ Soboul, *French Revolution*, 560; François Furet and Denis Richet, *The French Revolution*, Stephen Hardman, trans. (1965; London, 1970), 187.

¹⁴ Lefebvre, "La place de la Révolution dans l'histoire agraire," 516.

¹⁵ For example, Tom Kemp, *Economic Forces in French History* (London, 1971), 89 and *passim*; Robert Brenner, "Agrarian Class Structure and Economic Development in Pre-Industrial Europe," *Past and Present*, 70 (1976): 46, 74–75.

¹⁶ Florence Gauthier, *La voie paysanne dans la Révolution française: L'exemple de la Picardie* (Paris, 1977), and "Sur les problèmes paysans de la Révolution," *Annales historiques de la Révolution française*, 50 (1978): 305–14; Claude Gindin, "La rente foncière en France de l'Ancien Régime à l'Empire," *Annales historiques de la Révolution française*, 54 (1982): 1–34; Guy Ikni, "Sur les biens communaux pendant la Révolution française," *Annales historiques de la Révolution française*, 54 (1982): 71–94; and Hernani Resende, "Egalitarisme et question agraire dans la Révolution française," in *Contributions à l'histoire paysanne de la Révolution française*, Albert Soboul, ed. (Paris, 1977).

¹⁷ One of the more remarkable characteristics of the "revisionist" writing of the past twenty years is its lack of concern for the peasantry and rural social relations. See Martyn Lyons, "Cobb and the Historians," in *Beyond the Terror: Essays in French Regional and Social History, 1794–1815*, Gwynne

appear only in the penultimate chapter, where he claims they were not hostile to the *ancien régime* before 1789. (Doyle later argues that these hitherto “completely passive observers” took only three months to decide to overthrow feudalism entirely.)¹⁸ The three most extensively discussed “revisionist” overviews to appear in this bicentenary year, by Doyle, Simon Schama, and Furet, similarly concentrate on politics in the capital.¹⁹

The impetus to a redirection in theoretical and empirical work on the French countryside has come from three sources: economic history, non-Western agrarian history, and the work of the Soviet historian Anatolii Ado. The three are connected to each other in subtle ways that have not yet been fully explored.

First, as techniques for measuring nineteenth-century agricultural output increased in sophistication, economic historians using them found what they agreed was a significant increase in rural productivity in postrevolutionary France, although they disagreed about its timing. Michel Morineau, Hugh D. Clout, and Roger Price located a “breakthrough” in productivity only in the years after 1840.²⁰ Other historians, such as Maurice Lévy-Leboyer, George Grantham, and William H. Newell, identified gradual but important changes occurring much earlier in many regions.²¹ Such changes took the form of more intensive land use rather than the application of new technologies. In addition, according to Grantham’s recent study of a varied cross-section of rural France, proximity to towns, rather than farm size or soil type, was the crucial determinant of specialization. For example, as the population of Paris multiplied in the nineteenth century, so too did the amounts of fodder needed for the horses in the capital and its suburbs, which had increased from 16,000 horses in 1815 to 72,000 by 1874. Carts piled with manure returned from Paris to farms within a 120 kilometer-radius of the capital.²² Yosef Trounsky offered a somewhat different view of this growth, estimating that agricultural output increased by 20 percent during the Consulate and Empire and by 73 percent in the two ten-year

Lewis and Colin Lucas, eds. (Cambridge, 1983); and Soboul, “Historiographie classique,” 344–45. From this perspective, Alfred Cobban no longer seems “revisionist” enough, as George Comninel has pointed out in *Rethinking the French Revolution: Marxism and the Revisionist Challenge* (London, 1987), 18–20.

¹⁸ William Doyle, *Origins of the French Revolution* (Oxford, 1980), 209.

¹⁹ William Doyle, *The Oxford History of the French Revolution* (Oxford, 1989); Simon Schama, *Citizens: A Chronicle of the French Revolution* (New York, 1989); and François Furet, *La Révolution: De Turgot à Jules Ferry, 1770–1880* (Paris, 1989). In Schama’s book, there are more references to the Marquise de La Tour du Pin than to peasants.

²⁰ Michel Morineau, *Les faux-semblants d’un démarrage économique: Agriculture et démographie en France au XVIII^e siècle* (Paris, 1971); Hugh D. Clout, *The Land of France, 1815–1914* (London, 1983); and Roger Price, *The Economic Modernisation of France, 1730–1880* (London, 1975).

²¹ Jean-Claude Toutain, *Le produit de l’agriculture française de 1700 à 1958* (Paris, 1961); Jean Marczewski, “Some Aspects of the Economic Growth of France, 1660–1958,” *Economic Growth and Cultural Change*, 9 (1961): 369–86; Maurice Lévy-Leboyer, “La croissance économique en France au XIX^e siècle: Résultats préliminaires,” *Annales: Economies, sociétés, civilisations*, 23 (1968): 788–807; William H. Newell, *Population Change and Agricultural Development in Nineteenth-Century France* (New York, 1977); and George Grantham, “Agricultural Supply during the Industrial Revolution: French Evidence and European Implications,” *Journal of Economic History*, 49 (1989): 43–72.

²² Grantham, “Agricultural Supply during the Industrial Revolution”; see, too, Jacques Mulliez, “Du blé, ‘mal nécessaire’: Réflexions sur les progrès de l’agriculture de 1750 à 1850,” *Revue d’histoire moderne et contemporaine*, 36 (1979): 3–47.

Agricultural Production and Population 1815–1913

AGRICULTURAL PRODUCTION			POPULATION		
Years	<i>In Millions of Francs at 1905–13 Value</i>	<i>% Change per Annum</i>	Years	<i>In 1000s at End of Period</i>	<i>% Change per Annum</i>
1815–24	5,090	0.56	1815–21	30,462	0.59
1825–34	5,728	1.19	1821–36	33,541	0.61
1835–44	6,601	1.43	1836–46	35,402	0.53
1845–54	7,290	1.00	1846–56	36,039	0.18
1855–64	8,217	1.20	1856–66	38,067	0.53
1865–74	8,713	0.59	1866–76	36,096	–0.55
1875–84	8,356	–0.42	1876–86	38,219	0.56
1885–94	8,326	–0.04	1886–96	38,518	0.08
1895–1904	9,256	1.06	1896–1906	39,252	0.19
1905–13	10,265	1.04	1906–11	39,605	0.15

URBAN POPULATION

Year	<i>Towns over 2000 as %</i>	<i>Towns over 5000 as %</i>
1836	—	16.8
1846	24.4	—
1851	25.5	17.9
1856	27.3	—
1861	28.9	—
1866	30.5	24.4
1872	31.1	—
1881	34.8	—
1891	37.4	—
1901	40.9	35.6
1911	44.2	38.4

SOURCES: Agricultural production is from Jean Marczewski, "Some Aspects of the Economic Growth of France, 1660–1958," *Economic Development and Cultural Change*, 9 (1961): 369–86. Population figures after 1860 were inflated by the inclusion of the Comté de Nice and Savoie and deflated after 1870 by the loss of Alsace-Lorraine.

periods 1815–1824 and 1855–1864. Trounsky then linked the industrial expansion of France in the 1860s to the productivity of small-scale agriculture, drawing attention to the similar experiences of Germany and the United States in the late nineteenth century.²³

Jean-Claude Toutain and Jean Marczewski also argued that French agricultural productivity began to increase slowly before 1840, but they add that agricultural production accelerated after 1840 until it was on average three times faster than the rate of growth of the population for most of the nineteenth century (see Table).²⁴ In conjunction with the work of other economic historians, that of Toutain and Marczewski thus makes clear that, at a time when ownership or renting of small farms was increasing, agricultural output was growing

²³ Yosef Trounsky, "La Révolution française, modèle ou voie spécifique?" *Cahiers d'histoire de l'Institut de Recherches Marxistes*, 32 (1988): 207–15.

²⁴ Toutain, *Le produit de l'agriculture française*; Marczewski, "Economic Growth of France."

rapidly and more than satisfying the needs of expanding cities. Further evidence of the dynamism of the countryside appears in the work of Jean Pautard, who found that the volume of total crop production increased dramatically in the Limousin and in Brittany, two areas of small holdings, during the years 1840–1892, where the rate of growth was sharper than in the Paris Basin and the northeast.²⁵ While productivity per worker and per hectare was substantially higher throughout the period in the Paris Basin and the northeast, regions favored by greater soil fertility and open terrain, the fact remains that a substantial increase occurred in all four regions even though there was a growth in the number of small holdings. In all four regions, holdings under 40 hectares were over 90 percent of the total. This change in output on small holdings is best understood as an expression of the extension of “simple commodity production,” of which more will be said later in this article.²⁶

The economic historians’ challenge to entrenched notions about the negative and anti-capitalist effects of small-scale peasant agriculture has been strengthened from a second direction. Histories of recent rural change in non-European societies shatter a priori assumptions about how agricultural “modernization” takes place. Such assumptions, mediated through the prism of English agrarian history, have generated a whole library of learned works about “backward peasant agriculture,” and have served as the ideological basis for Western aid programs since World War II. More recent works show that in Japan, for example, new owners on 2 to 3-acre plots in the 1950–1963 period increased their output by 38 percent; the limited land reforms of 1952 in Egypt had similar effects. (These changes in part resulted from improvements in transport and greater access to credit and cooperatives, without which comparable growth did not occur in Bolivia and Iraq during the same period.)²⁷

Research that correlated social class and attitudes with agricultural change in eight countries, ranging from Kenya and Pakistan to Japan and the United States (Wisconsin), produces similar findings. Small farmers and peasants are far more often “innovative” in their growing strategies than those with substantial economic and political power.²⁸ As a result of such work, and, despite disagreement from powerful forces ranging from the World Bank to local elites, critics of conventional Western aid programs for developing countries increasingly identify those community-based programs that emphasize democratic decision

²⁵ Pautard, *Les disparités régionales*, 56–68, 83.

²⁶ The reasons for increased output on small holdings, and the consequent recasting of the conventional economic history of nineteenth-century France, are outlined in François Caron, *An Economic History of Modern France* (London, 1979), chaps. 1, 6; P. K. O’Brien, D. Heath, and C. Keyder, “Agricultural Efficiency in Britain and France, 1815–1914,” *Journal of European Economic History*, 6 (1977): 339–91; Peter McPhee, “A Reconsideration of the ‘Peasantry’ of Nineteenth-Century France,” *Peasant Studies*, 9 (1981): 5–25; and Georges Duby and Armand Wallon, *Histoire de la France rurale*, 4 vols. (Paris, 1975–76), vol. 3.

²⁷ Robert Forster, “Obstacles to Agricultural Growth in Eighteenth-Century France,” *AHR*, 75 (October 1970): 1600–15.

²⁸ Frank Cancian, *The Innovator’s Situation: Upper-Middle-Class Conservatism in Agricultural Communities* (Stanford, Calif., 1979).

making, political action, and regional self-sufficiency as most likely to improve rural living standards.²⁹

These policy-oriented studies suggest to the historian that English rural history be viewed as a specific case rather than as a paradigm for "development." For instance, Emmanuel Le Roy Ladurie and J. P. Cooper have used the examples of Flanders, other parts of France, the Netherlands, and Catalonia to contest Robert Brenner's argument that "England's uniquely successful overall economic development" contrasted sharply with the way that elsewhere "peasant agriculture set up [a] vicious cycle of backwardness thwarting agricultural capitalism."³⁰ Work on recent developments in non-Western countries has also stimulated interest in the historical role of small-scale agriculture in economic change and in the development of capitalism in the United States and Europe. Typical is Harriet Friedmann's analysis of Cass County, North Dakota, which found that self-employed and unpaid family labor was 78.5 percent of the agricultural labor force in 1909 and still 67.4 percent in 1960, despite major changes in farming techniques and output.³¹

IT IS TRUE THAT THE IMPLICATIONS of these first two developments are only slowly being applied to our understanding of the place of the revolution in the history of French rural society. But the impact of the third challenge to accepted generalizations about the peasantry, the revolution, and capitalism has been dramatic and rapid, at least among Marxist historians of the revolution. I refer here to the work of the Soviet historian Anatolii Ado, published in Russian in 1971 as *Krest'ianskoe dvizhenie vo Frantsii vo vremia Velikoi burzhuaiznoi revoliutsii kontsa XVIII veka* (The Peasant Movement in France during the Great Bourgeois Revolution at the End of the Eighteenth Century), and brought to the attention of French historians by Albert Soboul's lengthy résumé in 1973.³² While Ado has published only two general discussions of his work in French,³³ his thesis has been of seminal importance in suggesting a new relationship between the peasant movement and the revolution as a whole and a fundamentally new perspective on the importance of the revolution in the history of agrarian class structures. Georges Lefebvre's long-accepted overviews are being reworked in

²⁹ There is an extensive and growing critical literature on the politics and impact of "aid" and "development" programs: see, for example, Gerrit Huizer, *The Revolutionary Potential of Peasants in Latin America* (Lexington, Mass., 1972); Jon Bennett, *The Hunger Machine: The Politics of Food* (Cambridge, 1987); and Frances Moore Lappé, Joseph Collins, and David Kinley, *Aid as Obstacle* (San Francisco, 1980).

³⁰ Brenner, "Agrarian Class Structure," 63, 74–75; Emmanuel Le Roy Ladurie, "A Reply to Professor Brenner," *Past and Present*, 79 (1978): 55–59; and J. P. Cooper, "In Search of Agrarian Capitalism," *Past and Present*, 80 (1978): 20–65.

³¹ Harriet Friedmann, "Simple Commodity Production and Wage Labour in the American Plains," *Journal of Peasant Studies*, 6 (1978): 71–100; see, too, John Mack Faragher, *Sugar Creek: Life on the Illinois Prairie* (New Haven, Conn., 1986).

³² Albert Soboul, "Sur le mouvement paysan," reprinted in Soboul, *Problèmes paysans*, 117–34.

³³ Anatolii Ado, "Le mouvement paysan et le problème de l'égalité (1789–1794)," in *Contributions à l'histoire paysanne de la Révolution française*, Albert Soboul, ed. (Paris, 1977); "Bilan agraire de la Révolution française," *Cahiers d'histoire de l'Institut Maurice Thorez*, 27 (1978): 42–65. A full French translation of Ado's thesis is currently being prepared.

the light of both Ado's empirical findings and the new impulse he has given to a careful rereading of certain passages in Marx.³⁴

According to Ado, if capitalist agriculture was slow to develop in the nineteenth century, it was not because of small-holding and landless peasants tenaciously defending communal practices and archaic routines but because of the failure of small and middling peasants to achieve their goal of more land during the revolution. As Soboul explains it:

The persistence of large property being worked by sharecropping or small-scale leasing, and that of traditional landed rent, often aggravated in small-farming areas in the west and southwest by the survival of the tithe [*néo-dîme* or *dîme bourgeoise*], appears as a fundamental aspect. It is this large "retrograde" property that put a brake on capitalist penetration in the countryside, much more than small or middling producers, no matter what may have been the attachment of large sections of the peasantry to communal traditions. No doubt, in the process of the transition from feudalism to capitalism, large property was also significant, through the capitalization of land rent, but not without compromises and for a long time as an autonomous fraction of the new ruling class. This is explained both by the majority of the great landowners belonging to the former feudal aristocracy and by the existence of land rent as a particular form of transfer of the social product and of the distribution of surplus value. The rapid growth of capitalism in the French countryside in the nineteenth century thus would have required a greater extension of the sector of independent small and medium-scale production, this "rural democracy" of which [Jean] Jaurès spoke, the development of its potential, the total transformation of landholding relationships to the detriment of large retrograde property, as the occasion and condition for the free flowering of capitalism. Thus we come to Ado's essential conclusion: the negative aspects of capitalist evolution in nineteenth-century France come less from what the small and middling peasantry was able to impose on the bourgeois revolution (the survival of the rural community) than from what it was not able to snatch from it: the destruction of large property and the end of land rent.³⁵

The evidence found by Ado and others comes from the protests and rebellions going on in most of the countryside from 1789 to 1795. While extraordinarily diverse as a result of France's regional complexities, such actions all shared the same goals: freeing the land from the claims of "outsiders"—landlords, seigneurs, and the church—or increasing the land available to peasants. The second could be accomplished in a variety of ways: through subdivision of communal and national lands, through measures against engrossers (those who had annexed communal lands) and against middlemen in the rental market (*fermiers-généraux*), and through outright seizures and subdivision of large estates (the *loi agraire*). While the small sector of capitalist large-leaseholders (*grands fermiers*) would have suffered, more important would have been the destruction of large "retrograde" property.

Ado has indicated the contradiction between the anti-capitalist aspirations of the small peasantry and the objective consequences of their demands had they been realized.³⁶ A rapid extension of small and medium holdings would have

³⁴ As Soboul commented, on the centenary of Lefebvre's birth in 1974, "a historical study only remains alive if, read and contemplated, it is made more thorough. Scholarly research and critical reflection may then lead to a reconsideration"; Soboul, *Problèmes paysans*, 437.

³⁵ Soboul, "Sur le mouvement paysan," 133–34.

³⁶ Soboul, "Sur le mouvement paysan," 128.

accelerated the emergence of capitalist-proletarian social relations rather than simply encouraging greater consumption by peasants of their own products or even a retreat from the market. This is an important point, for the peasants who agitated for more land through the subdivision of commons, limitations on engrossers, or application of property seizures plainly did not do so for individualistic motives. Indeed, the movement to partition commons was often overtly anti-individualistic, calling for division by head or household, rather than in proportion to property already owned, and for community regulation. Many petitions called for divided commons to be inalienable for ten years or to be held on ten-year leases. As Lefebvre argued, the rural masses had a notion of legitimate or "customary" property that accepted private ownership so long as it did not transcend collective rights through enclosures or monopoly of resources.³⁷ However, despite such motivations, Ado argues that an expansion in the number of small and medium holdings would ultimately have speeded up class polarization in the countryside because of inequalities in resources, quality of land, and farming abilities.

Regrettably, few historians writing in English have commented on this new direction in rural studies, described by Claude Gindin at a special conference of the Institut de Recherches Marxistes in 1987 as "the decisive leap" in studies of the peasantry during the revolution.³⁸ Charles Warner's otherwise interesting critique concluded that Ado and Soboul were merely engaging in dialectical acrobatics, without mentioning the remarks Robert Forster had made the year before Ado published his thesis about the economically stultifying nature of most large holdings.³⁹ In Peter Jones's recent superb, judicious synthesis, which makes extensive use of Ado's research, the issue is explicitly raised but then shelved with the comment that, as the partition movement failed, Ado's thesis cannot be tested.⁴⁰ In contrast, David Hunt's challenging judgment⁴¹ on the extensive campaigns to force commons to be divided equally among all adults, often on short leases, is that this action stemmed from proto-socialist motivations; however, his argument does not consider what would have been the economic consequences of such land redistribution.⁴²

Within France, other Marxist scholars have continued to argue that the primary impulse to capitalism in the countryside was the strengthening of the sector of large tenant farmers in the eighteenth century and that Ado's stress on "the peasant route" to capitalism is overstated and unnecessary. Despite this

³⁷ Lefebvre, *Les paysans du Nord*, 70.

³⁸ In Trounsky, "La Révolution française," 7; see, too, Antoine Casanova and Claude Gindin, "Réflexions marxistes en mouvement: Du modèle à la voie spécifique," in *L'Image de la Révolution française*, Michel Vovelle, ed., 4 vols. (Oxford, 1989), 3: 1823–30.

³⁹ Charles Warner, "Soboul and the Peasants," *Peasant Studies Newsletter*, 4 (1975): 1–5; Forster, "Obstacles to Eighteenth-Century Growth," 1613–15.

⁴⁰ Peter Jones, *The Peasantry in the French Revolution* (Cambridge, 1988), 125–27.

⁴¹ David Hunt, "Peasant Politics and Communal Property during the French Revolution," *Theory and Society*, 17 (1988): 255–83.

⁴² On the complicated and surprisingly neglected question of the divergent political tendencies of peasant mobilizations, one of the themes of Ado's thesis, see David Hunt, "Peasant Politics in the French Revolution," *Social History*, 9 (1984): 277–300; and John Markoff, "Contexts and Forms of Rural Revolt: France in 1789," *Journal of Conflict Resolution*, 30 (1986): 253–89.

criticism, Ado and his French followers offer a strong theoretical justification for their position, one that is, moreover, supported by empirical evidence.

Ado's thesis has focused attention once again on arguments advanced twenty years earlier by Kohachiro Takahashi in his contribution to the debate in *Science and Society*, which followed the publication in 1946 of Maurice Dobb's *Studies in the Development of Capitalism*.⁴³ Takahashi argued that "in both England and France that [bourgeois] revolution had as its basis the class of free and independent peasants and the class of small and middle-scale commodity producers."⁴⁴ He saw the revolution as a struggle for state power: this middle class (best represented by the Jacobins) against a compromising *haute bourgeoisie* originating in the aristocracy and the merchant and financial world (Monarchiens, Feuillants, and Girondins).

While Takahashi's assessment of the urban revolution's social characteristics was quickly absorbed by Soboul and became the basis of his own contribution to the *Science and Society* debate, he and Lefebvre failed to grasp its implications for rural social relations.⁴⁵ But, during the last decade of his life, Soboul was increasingly drawn to this element of Takahashi's arguments and, through it, to some brief but important remarks by Marx and Lenin.

IN MARCH AND APRIL 1905, Lenin returned to comments Marx had made about the United States in 1846 as an analogy to a revolutionary agrarian program in Russia. Specifically, Lenin noted the parallels between Marx's attitude to Hermann Kriege's proposals for land redistribution in the United States and his own analysis of Russia in 1905. Supporting peasant struggles against landlords—like Kriege's utopian proposal to grant 160 acres to all households—would eventually advance class development in the countryside, even if initially appearing to retard it. Lenin predicted that "instead of a blow at grabbing speculators, you will witness the expansion of the basis for capitalist development . . . The revolutionary action of the lower class for a change that will temporarily provide a restricted prosperity, and by no means for all, will facilitate the inevitable further revolutionary action of the very lowest class for a change that will really ensure complete human happiness for all toilers."⁴⁶

In Volume 3 of *Capital*, Marx emphasized the "dual path" of the transition to capitalism: the "really revolutionising path," where small and middling direct producers in town and country became capitalists themselves, and the "way of compromise," whereby merchants integrated into previous socioeconomic struc-

⁴³ Kohachiro Takahashi, "Contribution to the Discussion," in *The Transition from Feudalism to Capitalism*, ed. Rodney Hilton (1954; new intro., London, 1976), 68–97.

⁴⁴ Takahashi, "A Contribution," 94.

⁴⁵ Albert Soboul, "Du féodalisme au capitalisme: Contribution," *La Pensée*, 65 (1956). In Lefebvre's own contribution to the debate in 1956, he failed to consider Takahashi's arguments: see Georges Lefebvre, "Some Observations," in Hilton, *Transition*, 122–27. For Soboul's post-1973 position, see *Problèmes paysans*, 9–22, and "Les paysans 'partageux' et la Révolution française," *L'Histoire*, 26 (1980): 30–37.

⁴⁶ Vladimir Ilich Lenin, *Collected Works* (London, 1960–), 8: 328.

tures contrived to keep industry subservient to merchant capital through strategies such as the putting-out method.⁴⁷ To apply this to France, the “really revolutionising path” of small-scale commodity production in town and country, “the historical premiss of the capitalist mode of production,”⁴⁸ was thwarted in postrevolutionary rural France by the persistence of prerevolutionary forms of rent and surplus extraction. The necessity followed for the poorer sections of postrevolutionary rural society to defend communal lands and customary rights as a way of survival.⁴⁹ At the same time, market-oriented farmers large and small often used commons as a precious *point d'appui*: they were not simply a remnant of the preindustrial rural community.⁵⁰

As my own contribution to this new direction in our studies of the revolution in rural France, I would suggest that Ado's thesis affords a way of integrating my earlier remarks about the changes in nineteenth-century French agriculture into the history of agrarian socioeconomic changes since 1789. That is to say, the economic, social, and ultimately political changes in the French countryside in the nineteenth century are best understood as a slow extension of “simple commodity production,” that “historical premiss” of capitalism, whose full capacity as the “really revolutionising path” was limited by the “retrograde” effects of large property rented in small holdings. The gradual and regionally varied transition from peasant agriculture to market-oriented small farming was the crucial economic and social change in rural France in the nineteenth century: indeed, for the end of the century, it makes little sense to talk about “peasant France” at all.

Critical to simple commodity production is the “commoditization” of production and reproduction. This takes place when households produce commodities (or exchange values) under conditions of competition and when independent producers are involved in the market not simply for the sale of surplus (as peasant producers) but for the exchange of almost all necessities (use-values) produced and consumed.⁵¹ Like peasant production, simple commodity production has a subsistence logic, the exchange of use-values (commodity–money–commodity) rather than the profit aim of capitalist farming (money–commodity–money plus surplus value). However, factors such as inequalities in the distribution of resources may result in simple commodity production becoming “profitable” and the basis of a capitalist transformation. While the peasant movement for land redistribution in the French Revolution was largely

⁴⁷ Karl Marx, *Capital*, vol. 3 (London, 1981), chap. 20, chap. 47, pt. 5.

⁴⁸ Marx, *Capital*, vol. 1 (London, 1976), chap. 32, appendix 1.

⁴⁹ Ikni, “Sur les biens communaux”; Soboul, *Problèmes paysans*, chap. 9; Cobban, *Social Interpretation*, chap. 10.

⁵⁰ Casanova and Gindin, “Réflexions marxistes en mouvement,” 1824.

⁵¹ There is an increasing body of material on this transitional form of production, reflecting the wider calling into question of Anglocentric assumptions about agrarian development: see, for example, Friedmann, “Simple Commodity Production”; Harriet Friedmann, “Household Production and the National Economy: Concepts for the Analysis of Agrarian Formations,” *Journal of Peasant Studies*, 7 (1980): 158–84; Jacques Chevalier, “There Is Nothing Simple about Simple Commodity Production,” *Journal of Peasant Studies*, 10 (1983): 153–86; and David Lehmann, “Two Paths of Agrarian Capitalism, or a Critique of Chayanovian Marxism,” *Comparative Studies in Society and History*, 28 (1986): 601–27.

thwarted, the removal of seigneurial dues, ecclesiastical tithes, and the sales of church and *émigré* property increased the proportion of produce retained by its direct producers and thereby created a safety margin on which peasants could risk greater market specialization. The gradual transition to capitalist agriculture in nineteenth-century France followed this “peasant route” to simple commodity production more frequently than it did large-scale capitalist tenant farming along the lines of the English model.

In one of the many examples of the transforming potential of simple commodity production, Philippe Vigier concluded, on the basis of his analysis of 600 southeastern communities, that “above all, *the extension of small property seems to be intimately connected with agricultural progress*” (his italics). In Valensole, for example, “The small proprietors of this commune bought up little by little, during the July Monarchy, a part of the land held until then in large holdings and left almost uncultivated . . . here, by 1847, fallow had almost completely disappeared, thanks to the introduction of sainfoin, and wheat had everywhere replaced rye—which only remained on the large scattered holdings of large proprietors.”⁵²

Olwen Hufton furnished an example of how the abolition of the tithe in 1791 markedly accelerated the transition from a diverse economy to market-oriented cattle raising in the countryside around the Norman town of Bayeux. The cathedral chapter of Bayeux—headed by a bishop whose position, one of the most lucrative in France, was worth 90,000 livres annually—had long been opposed to this transition, for tithe traditionally could be levied only on cereal crops. Tenants on church land were in fact required to grow grain to this end. By 1791, with the abolition of the tithe and the availability of huge quantities of church land, even small-holders and tenants could afford to take the risks of specializing in cattle raising.⁵³

Finally, in the southern village of Rodès, the respective roles of rural notables and small peasants also support Ado’s thesis. Land sales during the revolution had been monopolized by a local bourgeois, Joseph Cornet, whose son owned 25 percent of the land by 1835. Cornet used this land in long-established ways, to pasture sheep and to grow vegetables. His marriage to the daughter of an *émigré* noble in 1804, creating the Cornet de Candy lineage, is an archetypal example of the emergence of a family of conservative rural notables in the nineteenth century. In Rodès, the impetus for economic change came from independent small peasants producing wine and olive oil on some of the poorest land in the community. The particular socio-political ideology of these peasants—during the radical rural mobilization of 1849–1851, they were still advocating the “agrarian law” like the egalitarian fraction of the peasant movement of 1791–1793—suggests that, while their aspirations were utopian as well as

⁵² Philippe Vigier, *La Seconde République dans la région alpine: Etude politique et sociale*, 2 vols. (Paris, 1963), 1: 37. Similar conclusions are drawn for the Franche-Comté by Jean-Luc Mayaud, *Les paysans du Doubs au temps de Courbet: Etude économique et sociale des paysans du Doubs au milieu du XIX^e siècle* (Paris, 1979), chap. 3.

⁵³ Olwen H. Hufton, *Bayeux in the Late Eighteenth Century: A Social Study* (Oxford, 1967), chap. 2 and conclusion.

egalitarian, the realization of these aspirations would in practice have accelerated the expansion of capitalist social relations.⁵⁴

Soboul's own study of the Thomassin family of Puiseux-Pontoise (Seine-et-Oise, today Val-d'Oise) offers a capitalist transformation based on large-scale ownership or renting and the employment of "free" labor. The Thomassin family were owners of 3.86 hectares and lessees of 180 more in 1786. They bought up large amounts of national property and by 1822 owned 450.64 hectares (27.5 percent of the community's land). On these fields, cereals were alternated with fallow until, in 1856, Charles Thomassin built a distillery for treating sugar beets.⁵⁵ However, there are many other examples of the "retrograde" effects of large holdings, often produced by the "compromising" legislation of the revolution. For example, in December 1790, the National Assembly decreed that the value of the formerly levied tithe could now be added to rents.⁵⁶ Direct benefits from the revolution to sharecroppers and leasers were negligible—hence the power of the movement for the division of large holdings—and the value of exactions lost by seigneurs in 1789–1793 was simply added to new contracts. Although tenants had been able to profit from the dramatic inflation of 1795–1797 and buy land or acquit rents, this was a temporary gain. On average, rents increased by about 50 percent between 1798–1802 and 1817–1820.⁵⁷ As late as 1892, rented property still covered about 47 percent of the agricultural area of France, overwhelmingly in small plots, extracting on average about 15 percent of the surplus labor of tenant farmers.⁵⁸

Not only did many large landowners divert rents extracted from tenants and sharecroppers into unproductive consumption or status expenditure but, in many parts of France, contracts were extremely restrictive.⁵⁹ Peter Simoni's study of the district of Apt, for example, showed how, as late as the 1870s, sharecroppers had to agree to leases of fewer than five years, to share in kind harvest and vintage, to undertake a maze of unproductive and unpaid work, and even to forgo sowing consecutive crops or using commercial fertilizer.⁶⁰ Thus it was the nature and conditions of surplus extraction by large proprietors—remnants of prerevolutionary relations of production—that retarded capitalist agriculture and later sustained the defense of communal practices.

⁵⁴ Peter McPhee, "On Rural Politics in Nineteenth-Century France: The Example of Rodès, 1789–1851," *Comparative Studies in Society and History*, 23 (1981): 248–77. This recalls Gracchus Babeuf's criticisms of the impossibility of the "agrarian law" eliminating rural inequality. Lefebvre, while aware of the egalitarian movement, felt that it would have smothered rather than unleashed capitalism: see *Etudes sur la Révolution française*, 262; but note the comments of Gindin, "La rente foncière," 30–32. In any case, the nature of rural radicalism and socialism in the nineteenth century contradicts R. B. Rose's claim that the agrarian law was not only insignificant and reactionary but a dead issue by 1796: "The 'Red Scare' of the 1790s: The French Revolution and the 'Agrarian Law,'" *Past and Present*, 103 (1984): 128–29.

⁵⁵ Albert Soboul, "Concentration agraire en pays de grande culture: Puiseux-Pontoise (Seine-et-Oise) et la propriété Thomassin," *Problèmes paysans*, chap. 11.

⁵⁶ Jones, *Peasantry in the French Revolution*, 94–103.

⁵⁷ Albert Soboul, "Survivances féodales dans la société rurale du XIX^e siècle," *Annales: Economies, sociétés, civilisations*, 23 (1968): 965–86, reprinted in *Problèmes paysans*, chap. 7.

⁵⁸ Duby and Wallon, *Histoire de la France rurale*, 3: 466–67.

⁵⁹ Soboul, "Survivances féodales"; Gindin, "Rente foncière."

⁶⁰ Peter Simoni, "Agricultural Change and Landlord-Tenant Relations in Nineteenth-Century France: The Canton of Apt (Vaucluse)," *Journal of Social History*, 13 (1978): 115–35.

The significance of such new research into the rural dimensions of the French Revolution lies in its reintegration of the peasantry into the mainstream of revolutionary transformation. After decades of debates about often inconsequential “revisions” of the roles of the nobility and bourgeoisie, which relegated the peasantry to the position of conservative chorus to the main players, class relations in the countryside have returned to center stage, the place given to them by Georges Lefebvre sixty years ago, though in different guise. No longer portrayed as a monolithic, conservative rural bloc slowing the advance of bourgeois property relations and capitalist agriculture, the rural population now appears as a shifting amalgam of social groups whose shared vision of economic independence was partly realized, although constrained by the compromises after 1794 between successive governments and the large landowners of the *ancien régime*. At first sight, this interpretation of the French Revolution suggests *une voie spécifique* rather than a model of “the classic bourgeois revolution.” Yet the importance of small holdings in nineteenth-century French agrarian life has parallels in many European, South American, and Asian countries, as well as consequences for our general understanding of rural economics.

THE CONTEXT OF THE BICENTENNIAL of the revolution prompts two concluding points. The first concerns the implications of a properly historical—that is to say, processual—approach to agrarian social relations. Once we go beyond a simple descriptive stance toward small holding—which often assumes that small-holders are peasants and therefore unchanging—many of the apparently contradictory results of social scientists’ studies fit together, and the weaknesses of “modernization” approaches become readily apparent. Only a dynamic approach to agrarian social relations can explain, for example, the widely varying political behavior of rural communities and regions. Too many studies of French rural society based on the assumptions of “modernization” theory share a bemusing characteristic: each argues that the period it considers witnessed the “real” transformation of peasant society, whether the 1950s for Henri Mendras and Gordon Wright or the decades before World War I for Eugen Weber.⁶¹ It is hardly surprising that, based as they are on assumptions about a previously “traditional” and static society, these studies find change occurring and conclude that it is unprecedented.

“Modernization” theory is not really a theory at all but rather a tautological description of certain changes that are then clustered into a self-fulfilling prophecy. Significantly, it developed in the 1950s as an antirevolutionary response to wars of liberation in European colonies and now finds much favor in academe as a vulgarized form of monetarism. In its ethnocentric and presentist distortions of the past, it has parallels with Anglocentric assumptions about the

⁶¹ Henri Mendras, *The Vanishing Peasant: Innovation and Change in French Agriculture* (Boston, 1970); Gordon Wright, *Rural Revolution in France: The Peasantry in the Twentieth Century* (Stanford, Calif., 1964); and Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914* (Stanford, 1976).

proper route to “modernity.” These characterize even Marxist writing in English: hence George Comninel, in his recent discussion of debates about the Marxist interpretation of the revolution, ignored peasant agriculture except to summon it as evidence of France’s “backwardness.”⁶² Similarly, for Eric Hobsbawm, “the capitalist part of the French economy was a superstructure erected on the immovable base of the peasantry.”⁶³ Underlying such language is the assumption that the *raison d’être* of agrarian society should have been to increase productivity along English lines.

Second, what do the trends described in this article suggest? In re-reading the contributions to the debate on the state of our historical understanding of the revolution, I found the conclusions of one historian, written in 1973, particularly striking:

each concrete case offers specific characteristics that can only be reduced with difficulty to a model type of bourgeois revolution. Rejecting thus any recourse to a particular method or theory of the model, would it not be suitable to stop erecting the French Revolution as the “classic bourgeois revolution”? To underline better the irreducible and specific characteristics, should not one now qualify it as a “peasant-bourgeois revolution”? Questions one will only be able to respond to by theoretical reflection solidly supported by critical scholarship.⁶⁴

Given the frequency with which Albert Soboul is accused of repeating the same dogmatic clichés for forty years, it is remarkable that those remarks should be his. Indeed, they were written the same year as his final repetition of the argument that the revolution was “a classic bourgeois revolution,”⁶⁵ and represent a neglected but fundamental turning point in his whole approach. They serve to revive the hope that “revisionist” historians might convert their attacks on an oversimplified, even caricatured, version of Marxism into a more creative relationship between empirical research and theoretical considerations such as those outlined in this article.⁶⁶ But that may well be a naïve and vain hope. Despite Soboul’s challenge that every historian become a revisionist of received certainties, at the core of Marxist and “revisionist” debates remains a thorough-going, perhaps unbridgeable, political division.

⁶² Comninel, *Rethinking the French Revolution*, 189–93.

⁶³ E. J. Hobsbawm, *The Age of Revolution, 1789–1848* (New York, 1962), 213.

⁶⁴ Soboul, *Problèmes paysans*, 134.

⁶⁵ Soboul, *French Revolution*, 19.

⁶⁶ One such oversimplification of Marxist positions can be found in Robert Darnton’s otherwise illuminating, elegant discussion of bourgeois culture, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1984), 109–11. The political position of “revisionism” is well summarized in Comninel, *Rethinking the French Revolution*, 20–25. However, Comninel himself exaggerates the extent to which “revisionist” writing has recast historical understanding of the revolution; a more balanced view is that of Bill Edmonds, “Successes and Excesses of Revisionist Writing about the French Revolution,” *European History Quarterly*, 17 (1987): 195–217.

Revolutionary France, Cradle of Free Enterprise

JEAN-PIERRE HIRSCH

THAT THE FRENCH REVOLUTION LIBERATED PRODUCTION AND TRADE is a well-established notion, taught in France and accepted in the United States as well. English has adopted the French term “laissez-faire,” which, in the Library of Congress catalog, gives access to the richest array of material covering relations between the state and commercial or industrial enterprises. This linguistic preeminence is not the result of the clear superiority of French theoreticians of liberalism; France gave birth to Vincent de Gournay and Anne-Robert-Jacques Turgot, not to Adam Smith. Nevertheless, France was probably the first country where “laissez-faire” was officially accepted by the state and integrated into the new rhetoric of commerce and industry.

This article does not try to challenge the view that the French Revolution brought change in the relations between government and commerce. Instead, it addresses the as-yet-unanswered question of how this was done: through new policies, new institutions, a new discourse? The upsurge of laissez-faire notions that accompanied the revolution was so rapid, almost brutal, that not only lawyers and legislators but the general public as well, in particular, tradesmen and manufacturers, had difficulty understanding its implications. As a result of the revolution, the new business middle class had come closer to the sources of local and national power and had won social and political acceptance. Yet, despite such advances, it was unable to obtain or even to define a business policy that was entirely its own.

To explain this situation, we must enlarge our scope beyond legislative papers and Paris-based decisions. That is why part of the following analysis rests on a study of the Lille region. As the revolution advanced, merchants and manufacturers in this large trading area felt increasingly uneasy about the changes being introduced.¹ This article offers an account of what actually changed in this period, followed by some hypotheses about why these changes took place.

All my thanks to Danielle Michel-Chich, to the editors, and to W. M. Reddy, who worked hard to translate this article from French into English.

¹ J.-P. Hirsch, *Les deux rêves du commerce; entreprise et institution dans la région lilloise (1780–1860 environ)* (thèse de doctorat d'état, Lille, 1989), to be published by the Ecole des Hautes Etudes en Sciences Sociales.

FRENCH ENTREPRENEURS DOING BUSINESS BEFORE THE REVOLUTION were neither liberals nor interventionists. They did not go through a slow evolution from Colbertism to liberalism. Instead, they oscillated between the two philosophies, indulging until the last days of the Old Regime two dreams: a will to rule everything, which led the most aggressive traders to use all available institutional tools (such as protectionism and favorable regulations), and a vision of the possibilities offered by competition and free networks of trade.

These contradictory impulses help to explain the attitudes of the most powerful merchants toward the guild system. Their longstanding habit of ignoring the town guilds and making use of "free" labor from the surrounding countryside should not hastily be labeled hostility to corporate bodies. Leading merchants simultaneously supported the guild system, in which they were, after all, themselves engaged. In Lille, the hundred masters enrolled in the guild of *filtiers* (who produced twisted thread) in the second half of the eighteenth century included two groups. The majority owned only a few twisting mills and obtained their supplies either from the spinners in the town or, more commonly, from those in the countryside at the local market. They depended on merchants to sell their products. The second group, a few large-scale *filtiers* at the head of the guild, were their own merchants. They obtained their supplies directly from a variety of regions, including some outside of France, and controlled their own outlets, both near and distant. Large operators were not forced to be members of a guild by legal constraints. In fact, in a number of ways, they benefited from guild membership. Guilds ensured product quality and provided a familiar way of handling labor. But the large-scale merchants most likely remained satisfied with urban guilds because they also had access to the countryside, where labor was cheap and goods could be produced without guild regulation. Thus, for a long time, the merchant order relied on a dual system of production.²

Another example of the mixed nature of the prerevolutionary economy was the handling of bankruptcies. After a large increase in the number of bankruptcies during the 1780s, worried merchants demanded that bankruptcy cases be placed once again under the jurisdiction of the consular courts, whose magistrates were recruited entirely from within the trade. But merchants did not rely exclusively on their own authority to establish internal order. They repeatedly asked the government for help in protecting what they saw as an endangered business morality, for instance, by asking it to prosecute in cases of fraudulent bankruptcy. In these and similar situations, far from reproaching the government for its interference into affairs of trade, regional French merchants in the 1780s accused it of being too lenient with bankrupts and too prone to grant postponements (*surséances*) to merchants in difficult situations, thus interrupting proceedings against them. Merchants attempting to protect the system of credit still felt they needed the power of the government as well as their own efforts.

² Archives municipales, Lille, 666. Also see J.-P. Hirsch, "Négoce et corporations," in *La Révolution française et le développement du capitalisme*, special issue of *Revue du Nord*, 1989, G. Gayot and J.-P. Hirsch, eds. The example of Lille corporations is studied in further detail by Gail M. Bossenga, *Corporate Institutions, Revolution and the State: Lille from Louis XIV to Napoleon* (Ph.D. dissertation, University of Michigan, 1983).

That attitude reflected a diffidence among the new men of trade who, until the first days of the revolution, remained unsure about the character, and even the dignity, of their work. In virtually all of industrial and commercial France at the end of 1788, chambers of commerce and commercial courts asked for special representation in the Estates General; a kind of unhappy uncertainty characterized even the speeches of the representatives of the big harbors on the Atlantic coast.³

Of course, there was rapid economic growth in the eighteenth century, which had definite effects on the thinking of merchants and manufacturers. This can be seen in two very different trends. First, the increasing scale of trade led merchants to support the elimination of custom barriers and the standardization of money units and units of measure in all the lands and seas where the king was sovereign. But a change of scale brought no revolution in habits, for French merchants became more anxious than ever to preserve the traditional strict colonial system of exclusive markets. When the monarchy timidly attempted to loosen market prohibitions in 1786, with the signing of the Anglo-French Commercial Treaty, most manufacturing towns reacted with hostility; they continued to complain until the revolution broke out.

Second, in many towns, the conflict between merchants and artisans, that is, between merchants and the home workers they employed, sharpened, especially from the 1760s on. During this newest expression of a longstanding conflict, the merchant urban elite benefited from the opportunities offered by the language of the Enlightenment, especially its condemnation of guild privileges. As early as 1718, the Lille chamber of commerce expressed frustration about "the problems created by certain privileges constituted for the welfare of trade and manufactures," in particular, the way some wool workers were sheltered by their statute of *ouvriers-francs*.⁴ But such criticisms were sporadic and unsystematic. Except in times when unruly artisans entrenched themselves within their guilds, merchants remained deeply loyal to the existing trade structure. Moreover, they never considered doing without the administration and regulation of trade.

Such examples make clear that, before 1789, the theory of laissez-faire was not much favored by merchants. The Physiocrats, whose propaganda used Gournay's slogan of laissez-faire, in no way took into account the interests of trade, let alone the special interests of merchants, and attributed the increase in merchant wealth to their excessive use of monopoly.⁵ Although the spreading of "liberalism" (before the word itself appeared) was remarkable in post-1750 France, calls for economic reforms, including tax reform, should not be interpreted as expressions of discontent on the part of a business middle class. Turgot, encouraged by the prospect of opening up the country, considered free

³ J.-P. Hirsch, "Les milieux du commerce, l'esprit de système et le pouvoir à Lille à la veille de la Révolution," in *Annales: Economies, sociétés, civilisations*, 30^e année (November–December 1975): 1337–70.

⁴ Archives départementales du Nord, 76 J 1 *.

⁵ For François Quesnay, the trader was "a stranger in his own country": *Quesnay et la Physiocratie*, annotated texts (Paris, 1958), 2: 827; G. Weulersse, *Le mouvement physiocratique en France de 1756 à 1770* (1910; rpt. edn., New York, 1968), 309, 588 and following.

enterprise to be the only cure for a chronic disequilibrium in agriculture, resulting from variations in weather and in the fertility of the soil. His sort of liberalism, which condemned barriers and taxes but praised and promoted the organization of almost everything, did not encourage individual initiative or criticize the invasive actions of the state. It considered only the “political body,” where the circulation of money played the role of “the circulation of the blood in a living body.”⁶ Even the most doctrinaire formulation of the new principles, the *Essai sur la liberté du commerce et de l'industrie* of Bigot de Sainte-Croix, published posthumously in 1775 in support of Turgot's reforms, carefully distinguished between the guilds (whose dissolution it recommended) and the precious regulations “adopted to regulate the trade and to determine the quality of works” (which were to be kept).⁷ Adam Smith's *Wealth of Nations* (1776) was much acclaimed in France, where it was translated and reprinted several times within a short period. But, in the liberal experiments conducted between 1770 and 1786, the beneficial effects of “the invisible hand” were in no way visible. Instead, these experiments aroused a strong opposition; the most active entrepreneurs lacked words harsh enough to express their criticism of the “systematic minds” behind them.

THE FRENCH REVOLUTION INTRODUCED free enterprise abruptly. By the end of the revolutionary decade, the term “freedom of trade,” which had long served as but one of many catch phrases merchants used to advance their disparate purposes, became an unchallengeable doctrine. But enthusiasm was not the only response to this new doctrine, and many who invoked it did so only with difficulty. For discourse was made unequivocal only by ignoring the contradictions of practice, which had in fact changed very little. Resulting difficulties can be seen in trade legislation and in what one twentieth-century scholar has referred to as the “congenital disease” of commercial law: its inability to provide long-term solutions to several particularly irritating problems, such as bankruptcies and the regulation of inventions.⁸

Were these especially difficult policy issues to solve? Did this “disease” derive directly from the new common use of “free trade”? The events of the first decades of the nineteenth century provide partial answers. During that period, merchants and manufacturers found that they had to do quite a bit of juggling of words to continue espousing individual enterprise and freedom of trade. They invoked the great principle of freedom of trade in every complaint about the authorities, and there were plenty of occasions to resist proposed taxes, controls, and inquiries. But uneasiness arose each time the banner of free trade was used to justify what, in earlier periods, would have been explained quite

⁶ Anne-Robert-Jacques Turgot, *Réflexions sur la formation et la distribution des richesses* (Paris, 1766), vol. 68; *Oeuvres de Turgot*, ed. Eugene Daire (Paris, 1844), vol. 1, 45.

⁷ Bigot de Sainte-Croix, *Essai sur la liberté du commerce et de l'industrie* (Amsterdam and Paris, 1775).

⁸ J. Hilaire in Alain Sayag, et al., *Quel droit des affaires pour demain? Essai de perspective juridique* (Paris, 1984).

differently. For instance, “freedom of trade” was used to justify the political and social advantages gained by the merchants as a result of the revolution. Yet, before the revolution, merchants had climbed the social ladder by accepting and working within the complex edifice of privilege. And, since the revolution’s outbreak, their advances were linked only indirectly to the changes occurring in the organization of trade. Still, the business class acted as if it was obligatory to attribute all good that came their way to freedom of trade. For example, Roubaix manufacturers continued to ask the central authority to approve their manufacturing regulations. But, finally, they began to consider as a fortunate outcome of “laissez-faire” policy their ability to make their own collective regulations without interference.⁹ The notion of liberty constrained its adherents to use a discourse charged with denials and excuses. Books on commercial legislation published in the first half of the nineteenth century are remarkably clever when it comes time to review the still abundant regulations and restrictions in force; these are only “special rules,” we are told, listed in flexible and complicated but incomplete ways. The treatise of J.-M. Pardessus, for example, gives an entire page of “special regulations in the public interest” and does not even quote “a great many others it would be too long to enumerate.” According to him, however, such regulations were only “exceptions” under the absolute reign of freedom of trade.¹⁰

The *individual* nature of modern enterprise was the other main element of the new rhetoric, although here, too, it is doubtful that this cliché truly expressed the early nineteenth-century attitude. Indeed, newcomers continued to start their own industrial and commercial ventures. But, for most of them, the venture proceeded only if it fitted, sooner or later, into the stable network formed by the great merchant families.¹¹ While the existence of these families is well known, historians have not sufficiently emphasized their power, from the first half of the nineteenth century, as functioning governors of manufacture. Supported by a municipality or an industrial tribunal (*conseil de prud’hommes*), they discreetly organized durable agreements in the silk industry in Lyons and in the weaving industry in Roubaix. The manufacturers in Roubaix consulted each other throughout the 1820s on the definition of their products, trademark, and even prices.¹²

One might argue that these unofficial agreements are a significant detail of economic life in postrevolutionary France. Relations between enterprises and governments are often intricate or strained in other countries as well. But the

⁹ Archives départementales du Nord, 79 J 35. The same behavior took place in Lyons; see Alain Cottureau, “Justice et injustice ordinaire sur les lieux de travail d’après les audiences prud’homales (1806–1866),” *Le Mouvement social*, 141 (October–December 1987): 53–54.

¹⁰ Jean-Marie Pardessus, *Cours de droit commercial*, 6th edn., 4 vols. (Paris, 1856), 1: 111. Also see Emile Vincens, *Exposition raisonnée de la législation commerciale et examen critique du code de commerce*, 3 vols. (Paris, 1821), 3: 44 and following.

¹¹ G. Gayot, “Le point sur le renouvellement des dynasties d’entrepreneurs à travers la Révolution,” in *La Révolution française et le développement du capitalisme*.

¹² Developed in Hirsch, *Les deux rêves du commerce*, chap. 13. On Lyons, see Cottureau, “Justice et injustice.” An attempt to list all these agreements in the early nineteenth century is in Bertrand Gille, *Recherches sur la formation de la grande entreprise capitaliste (1815–1848)* (Paris, 1959), 129–62.

French people refused to admit the reality of the “intermediary interests” that united their entrepreneurs. In 1861, for example, in order to go to court as a body in a case against illegal imitations, the Roubaix manufacturers had to ask the ministry of the interior for permission to associate; that authority was granted as long as the association was limited to “the defense of the interests of each member.”¹³ Where else in the world could such an absurd distinction be made? In his study, *The Foundations of American Economic Freedom*, E. A. J. Johnson argued that, in the United States in the late eighteenth century, “interest” often was used as a collective term. According to Johnson, the framers of the Constitution intended to harmonize the various interests of groups and types of trades as much as those of individual citizens.¹⁴ In France, however, the power of notions of laissez-faire and of the individual nature of enterprise made it difficult to develop a good business policy, for discourse and practice were far apart, and the revolution only widened the gap.

THE QUESTION REMAINS, THEN, WHY DID THIS GAP come into existence? One may offer at least a few clues to the answer. The real year of the birth of free enterprise was 1791, for, between February and September of that year, the Constituent Assembly passed three decisive decrees on trade after deputies Pierre d’Allarde, Isaac Le Chapelier, and Pierre Goudard made their reports. But the logic of those decrees is clear only from hindsight: a fairly coherent strategy to dissolve corporations progressively was followed abruptly by the decision to abolish all the administration of trade and the machinery of the law. This process began during the period from February to June, when the Constituent Assembly considered, then adopted, the banning of associations of “citizens of the same condition and trade.” The d’Allarde decree, which banned the guilds, concluded a debate on fiscal matters; then, in June, the Le Chapelier decree put an end to all associations under the name of “so-called common interests” with a constitutional principle: “There no longer are corporations within the state; there is only the particular interest of each individual and the general interest. From now on, no one will rouse citizens to associate for an intermediary interest or use a corporatist spirit [*esprit de corporations*] to separate them from the public interest.” But, on September 27, the Goudard decree led the French economy in another direction, abolishing the administration of trade. The regulation of manufactures, clearly stated to be necessary in February and not questioned in the two previous decrees, officially disappeared. The famous Article 7 of the d’Allarde decree, “Any person will be free to choose the trade, profession, art or occupation he wants . . . ,” was supplemented by a less famous amendment: “. . . as long as he follows the various regulations that may be

¹³ Archives municipales, Roubaix, F II (a) no. 1.

¹⁴ E. A. J. Johnson, *The Foundations of American Economic Freedom: Government and Enterprise in the Age of Washington* (Minneapolis, Minn., 1973), 335.

adopted.”¹⁵ But the Goudard decree, passed in the last days of the Assembly, banned the administration, the offices, and the inspectors of manufactures.

When the anti-corporation policy was thus coupled with an anti-regulation policy, two issues carefully kept separate from the time of Turgot until the beginning of 1791 were finally merged. The Constituent Assembly took this action out of an urgent desire to solve the problem before their mandate ended. Actually, Goudard had not proposed abolishing but, rather, reorganizing the administration by introducing a new balance between local and central authority. Overworked and with many unsolved questions before them, the deputies simply approved the articles that abolished the old system. They expected their successors to discuss a new one.

This accident in history is not sufficient to explain why the abolition of regulations was not reconsidered more clearly later on. From the Legislative Assembly to the Directory, every attempt to reestablish an official administration of trade failed, leaving the same institutional void. Such an administration finally was set up under the Consulate and the Empire, within the ministry of the interior and locally, either using new local institutions (such as consultative chambers of manufactures and industrial tribunals) or restoring old ones (chambers of commerce). The 1803 law (Germinal an XI) reestablished the major part of the regulatory framework of manufactures. But, by then, the gap between practice and doctrine was well established. The new institutions worked in a strange relationship with the principles of free trade (which were then triumphant) and sometimes even led a semi-clandestine existence: the chambers of commerce, for instance, which offered some of the best examples of assemblies of “citizens of the same condition and trade,” and which had been abolished from September 1791 to December 1802 (Nivôse an XI), were set up again, but they played a much more influential role than the consultative one officially granted to them. Many regulations were reestablished or created (mainly at the municipal level) and constituted as “exceptions” to a general system of laissez-faire, thus not threatening its anti-corporatist and anti-regulatory dimensions.

A case study of French merchants involved in the transition to free enterprise offers some clues as to why the business middle class failed to impose an economic program of its own, and allowed the law to continue in a state of incoherence, even when revolutionary events brought it to the forefront of power. Here, only a rapid overview of research carried out on the Lille area is possible. In Lille, those merchants who placed great confidence in the revolution at the beginning were quickly disappointed. As early as 1789, they thought it possible to institute not laissez-faire but a concrete policy in which the progress made by political freedom and a constitutional monarchy would improve credit and help to develop industry. The nation, at last organized, would succeed in getting rid of its debt and in establishing a new order for credit and production.

¹⁵ The texts of the three decrees are in *Archives parlementaires de 1787 à 1860, recueil complet des débats législatifs et politiques des Chambres françaises*, 1st series, 23: 198 and following; 27: 210 and following; 31: 396 and following.

But, in actuality, the community of merchant-citizens was not able to overcome its internal conflicts. For example, the merchants did not succeed in solving the problem of bankruptcies in the commercial courts, although those courts were given back all their powers in that area. The new bills of entitlement (*assignats*) provided the most severe disappointment, especially as they had first been enthusiastically considered the perfect means of harmonizing commercial and public credit on the English pattern. Even though many Lille merchants and manufacturers finally benefited from the *assignats*, acquiring secularized church lands and buildings on a large scale, this positive result did not help to ease their minds. In fact, the outcome ironically confirmed the continued importance of the most traditional basis of trade: land and political favor.

The conversion to free enterprise was not the expression of the solid position of a social group clearly stating the new conditions of its development; on the contrary, it took place among disappointed people whose reforming task had been interrupted. By the time the Assembly passed the new legislation in Paris in the 1790s, the enthusiasm of the early days of the revolution had waned. The year 1791 saw the flight of the king to Varennes and the Champ de Mars massacre; on these occasions, tensions between the middle class and the people's movement continued to grow. The abolition of corporations, an idea that had slowly progressed in delegates' minds from the summer of 1789, came as a riposte to a sudden eruption of threats coming from Parisian workshops. In Lille, the leading merchants gradually came to appreciate the advantages of the condemnation of corporate bodies and intermediary interests in their struggle to reestablish order among artisans seduced by the sans-culotte spirit, though they first vigorously refused the abolition of regulations. The decree passed in September 1791 roused a strong campaign in defense of manufacturing regulations, trademark offices, and supervisors (*égards*) in Lille. In the spring of 1792, all the important textile leaders united around this issue and signed a petition. According to its text, the existence of guilds was "a real blow to freedom and industry." But the regulations, "whose purpose was to ensure the prosperity of manufacturers and to prevent frauds . . . could and should be kept."¹⁶

Other sporadic struggles took place; but, after a certain period of reaction, the Lille traders adapted themselves to this second dimension of free enterprise. They acquiesced more out of resignation than of certainty, even if, as elsewhere, in so doing they adopted the themes of the new rhetoric. The notion of free trade was to prove very useful in social relations in a city that was becoming one of the main centers of the industrial revolution. The petition of 1792 expressed the fear of seeing "manufacturing exposed to the workers' shrewdness without regulations."¹⁷ But, gradually, manufacturers became aware of the advantages offered by institutions whose rules, like those of the industrial tribunals, stated that only individuals could be involved in contractual relations. This principle, they finally realized, depriving workers of the right to act collectively, left workers more submissive to employers' authority. Yet manufacturers did not

¹⁶ Archives municipales, Lille, 17.997.

¹⁷ Archives municipales, Lille, 17.997.

give up their practice of using collective regulations at all levels, whenever beneficial to them, or of appealing for help to the various powers, mainly those within the municipal governments of manufactures.

At the end of the revolution, then, representatives of trade and industry were attempting to master two problems—credit in exchanges and authority over laborers—using contradictory means, freedom or regulation, as the case might be. But it was difficult to say this or even to think it.

Historians can agree on the birthplace of free enterprise: revolutionary France. But the identity of the child seems much more problematical. And this problem leads to two much larger issues. The first concerns the long-term development of commerce and industry. What faith should we put in those forms of discourse that hide from view the ordinary contradictions of business practice? For the dual dream of trade is founded on trade's fundamental, if contradictory, needs: to conquer new markets while limiting the ravages of competition, to innovate and create new products without breaking with those norms that bolster the confidence of the consumer and thus ensure continued sales. It is quite difficult to believe that there was an epoch of true "free enterprise" that came between the Old Regime and the later emergence of "monopoly capitalism." Did the nineteenth century really bring anything other than the success of what William R. Reddy has called "market culture"? And did the "Great Transformation" analyzed by Karl Polanyi really mark the end of an epoch or only the end of an illusion?¹⁸

These remarks lead, finally, to a general question concerning the history of the French Revolution. It brought the sudden adoption of a new order, or rather of a new economic disorder, brimming with possibilities for future conflict, expressing as well the coming to power of a new bourgeoisie. But must one not also insist on the limits and the incoherence of this bourgeois revolution?

¹⁸ William M. Reddy, *The Rise of Market Culture: The Textile Trade and French Society 1750–1900* (Cambridge and Paris, 1984); Karl Polanyi, *The Great Transformation* (New York, 1944).

Racial Equality, Slavery, and Colonial Secession during the Constituent Assembly

DAVID GEGGUS

THE COLONIAL QUESTION IN THE FRENCH REVOLUTION involved three broad issues: self-government for France's overseas possessions, civil rights for their free colored populations, and the abolition of the slave trade and slavery itself. This article is primarily concerned with the pursuit in France of racial equality and slave emancipation, but it is difficult to understand that effort without reference to developments in the colonies, as well as to the contemporary debate about the limits of metropolitan control and the threat of white secessionism within the empire.¹ Indeed, one of the chief interests of the colonial question lies in the interaction of its three component issues and the complex counterpoint that developed between events in Europe and the Caribbean.

Until recently, the colonial question has received remarkably little attention from scholars of the French Revolution. As Mitchell Garrett observed in 1916, historians have been less interested in French attitudes toward the colonies during the revolutionary period than in the colonies themselves.² The abolition of slavery in 1794, surely one of the most radical acts of the entire revolution, gets no mention in the classic studies of Jules Michelet, Jean Jaurès, Albert Mathiez, and Albert Soboul, nor in the recent histories of George Rudé, D. M. G. Sutherland, and Simon Schama.³ Matters of empire, race, and slavery fail to appear in the documentary collections of J. M. Roberts and John Hardman, or in such different works as those of Peter Kropotkin, Pierre Gaxotte, and (barring a misleading half-sentence) Alexis de Tocqueville.⁴ Even

¹ The distant Indian Ocean colonies of Île de France and Bourbon were less affected by the revolution than were the West Indian islands, and they had little impact on revolutionary politics. Of the Caribbean colonies, Saint Domingue was by far the most important economically and as a foyer of revolution.

² Mitchell B. Garrett, *The French Colonial Question, 1789–1791* (New York, 1916), iii.

³ George Rudé, *The French Revolution after 200 Years* (New York, 1988); Donald M. G. Sutherland, *France, 1789–1815: Revolution and Counterrevolution* (New York, 1987); Simon Schama, *Citizens: A Chronicle of the French Revolution* (New York, 1989).

⁴ J. M. Roberts, ed., *French Revolution Documents* (New York, 1966); John Hardman, ed., *French Revolution Documents*, vol. 2. (Oxford, 1973); Pierre Kropotkin, *La Grande Révolution, 1789–1793* (1893; Paris, 1909); Pierre Gaxotte, *La Révolution Française*, nouv. edn. (Paris, 1975); Alexis de Tocqueville, *L'Ancien Régime*, ed. G. W. Headlam (Oxford, 1904), 269. Although Tocqueville gave the impression that the aristocracy's *cahiers* called for ending slavery and the slave trade, only a tiny number did so.

the wide-ranging surveys of Jacques Godechot and Robert R. Palmer accord the briefest of mentions to France's colonies.⁵

This neglect is surprising in view of the importance to *ancien régime* France of its overseas possessions. The expansion of French foreign trade in the eighteenth century was more rapid than that of Great Britain, and it was fueled largely by the Caribbean colonies of Martinique, Guadeloupe, and, most important, Saint Domingue (modern Haiti). By the late 1780s, they produced about half the Western world's sugar and coffee, and three-quarters of this produce was reexported from France, earning vital foreign exchange. Perhaps more than a million of the French depended on colonial commerce for employment in what was the most dynamic and advanced sector of the French economy. The demise of the Atlantic coast ports during the revolution was in part a product of colonial problems, and Caribbean developments similarly contributed to *la vie chère* on the streets of Paris, where sugar riots broke out in 1792.⁶

The neglect of colonial affairs in French revolutionary historiography is also surprising because of the significance of the issues raised by the colonial question. The world's first examples of colonial representation in a metropolitan assembly, of racial equality in an American colony, of wholesale emancipation in a major slaveholding society, and of the exportation of these policies as weapons of war all date from the years 1789–1794. In the political rhetoric of the mother country, slavery was merely a metaphor,⁷ but it was a grim reality in the colonies of the Caribbean and Indian Ocean. The emancipation decree of February 4, 1794 (16 Pluviôse Year II), legally freed some 700,000 people, without compensating owners for the 1,000 million livres tournois in capital investment they represented.⁸

It is true that the French themselves preferred at the time to avoid the embarrassing problems posed by the colonies. Colonial autonomy, racial equality, abolition of the slave trade, and slave emancipation were grave threats to French prosperity, threats that the Declaration of the Rights of Man appeared rashly to promote. The colonial question thus tested the universalist claims of the French revolutionaries, and, according to Jaurès, it sapped the self-respect of the revolutionary bourgeoisie, forced into a painful confrontation of principle and interest. Foreshadowing the imperial crises of the present century, it is for Aimé Césaire the crucial question of the revolution.⁹ Its neglect by historians of France is therefore all the more surprising.

⁵ Jacques Godechot, *La Grande Nation* (Paris, 1956); Robert R. Palmer, *The Age of the Democratic Revolution*, 2 vols. (Princeton, N.J., 1959, 1964).

⁶ Robert Stein, *The French Sugar Business* (Baton Rouge, La., 1988); Noel Deerr, *History of Sugar*, 2 vols. (London, 1949–50); Jean Tarrade, *Le commerce colonial à la fin de l'Ancien Régime* (Paris, 1972).

⁷ In William Doyle, *The Oxford History of the French Revolution* (Oxford, 1989), four of the ten indexed entries under "slavery" refer to metaphorical usage.

⁸ Published population statistics are very unreliable; they are investigated in David Geggus, "The Major Port Towns of Saint Domingue in the Late Eighteenth Century," in Franklin Knight and Peggy Liss, eds., *Atlantic Port Cities: Economy, Culture, and Society*, forthcoming. Newly arrived African adults sold in Saint Domingue for close to 2,000 livres tournois in 1789. Locally born and skilled slaves could be worth considerably more, but children and the aged were less valued, and prices were somewhat lower in the other colonies.

⁹ Jean Jaurès, *Histoire socialiste, 1789–1900*, 6 vols. (Paris, 1924), 1: 574; Aimé Césaire, *Toussaint-Louverture: La Révolution Française et le problème colonial* (Paris, 1962), 21–22.

Yves B  not has suggested that this lack of attention may be partly attributable to the imperialist sensibilities of early writers, who preferred to overlook the struggles of the colonized. Their doing so influenced later historians, who have unwittingly absorbed their definition of the revolution's agenda.¹⁰ Whether or not this is so, at issue here are perceptions of the meaning and boundaries of the French Revolution.

DATING BACK TO JEAN BODIN, ANTISLAVERY IDEAS had a long history in France but remained at the end of the *ancien r  gime* little more than a superficial embellishment of Enlightenment thought. The inhumanity of slavery had been condemned by Montesquieu, Voltaire, Rousseau, and contributors to the *Encyclop  die*, although with varying degrees of ambiguity. Even those like Denis Diderot and Louis-S  bastien Mercier, who called for violent self-liberation, showed no sustained commitment. Although the Physiocrats had attacked slavery's economic utility incidentally, their arguments made little headway against an institution so vital to the expansion of French commerce.¹¹ The presence of slaves in France was exploited frivolously in *parlementaire* battles with the crown at the same time that explicitly racist attitudes became more prominent in scientific and government circles.¹²

Despite such obstacles, a new practical antislavery concern emerged in the 1780s with the publication of concrete schemes for gradual abolition by the marquis de Condorcet and by the abb   Raynal, in his best-selling *Histoire des deux Indes*. Jacques Necker suggested an Anglo-French initiative to abolish the slave trade. Lafayette sponsored a secret emancipation experiment on a plantation in Guiana. Also in secret, the colonial minister de Castries pondered schemes for reforming and eventually abolishing slavery.¹³ In February 1788, Jacques-Pierre Brissot founded the elite Soci  t   des Amis des Noirs, which called for the ending of the slave trade and the eventual and gradual, but uncompensated, abolition of slavery.¹⁴ Unlike Britain, however, France never developed a popular abolitionist movement.¹⁵

¹⁰ Yves B  not, *La R  volution Fran  aise et la fin des colonies* (Paris, 1988), 205–17.

¹¹ David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), chaps. 13–15; William B. Cohen, *The French Encounter with Africans* (Bloomington, Ind., 1980), 130–54; Roger Toumson, ed., *La p  riode r  volutionnaire aux Antilles* (Schoelcher, Martinique, [1989]).

¹² Pierre Pluchon, *N  gres et Juifs au XVIII   si  cle* (Paris, 1984), 148–57, 246–47, 263–83; Pierre Boule, "In Defense of Slavery: Eighteenth Century Opposition to Abolition and the Origins of a Racist Ideology in France," in Frederick Krantz, *History from Below* (Oxford, 1988), 219–46; Cohen, *French Encounter*, 60–99.

¹³ B  not, *R  volution*, 21–41; Gabriel Debien, *Les esclaves aux Antilles fran  aises aux XVII   et XVIII   si  cles* (Basse-Terre, 1974), 488–90.

¹⁴ Jacques-Pierre Brissot, *Discours sur la n  cessit   d'  tablir    Paris une soci  t   pour concourir . . .    l'abolition de la traite et de l'esclavage des N  gres* (Paris, 1788). For lack of documentation, the Soci  t   has been the subject only of sporadic, article-length studies; most recently Fran  oise Th  s  e, "Autour de la Soci  t   des Amis des Noirs," *Pr  sence africaine*, 125 (1983): 3–82. However, the appearance of the Soci  t  's *proc  s-verbaux* in the 1982 sale of the Brissot de Warville papers is encouraging for future research. For an incisive sketch of the group's deficiencies, see David Brion Davis, *The Problem of Slavery in the Age of Revolution* (Ithaca, N.Y., 1975), 95–100.

¹⁵ Seymour Drescher argues that a mass base was vital to the British antislavery movement, although in France abolitionism eventually triumphed in the nineteenth century without gaining

Nearly fifty of the 520 surviving general *cahiers* expressed hostility to slavery or the slave trade, although many of these also cautiously requested a compromise between “political interest” and “the sacred rights of humanity [or, liberty].”¹⁶ In his long opening speech to the Estates General in 1789, the king’s minister Necker also enjoined the deputies to consider the plight of black slaves. And, in a remarkable pamphlet published that same year, Brissot argued that the deputies owed “as much to [their] fellow citizens of the colonies as to those of Europe, as much to black Frenchmen as to white.”¹⁷

The very first question on which the united three Orders had to vote in 1789 was the admission of illegally elected white representatives from the West Indies. After some delay, colonial representation was voted by acclamation. The one deputy who argued that colonies were not part of the *patrie* was quickly silenced. To the planters’ discomfort, however, these discussions enabled some prominent deputies, notably Mirabeau and La Rochefoucauld, to bring up the issues of slavery and racial equality.¹⁸ During the early summer of 1789, the colonial question did seem to be part of the revolutionary agenda.

Yet the Amis des Noirs were to meet complete failure in the Constituent Assembly. With hindsight, this looks like a foregone conclusion, but some contemporaries were optimistic. It seemed to the abolitionist Thomas Clarkson, arriving in Paris in early August 1789, that the Assembly might abolish the slave trade by acclamation.¹⁹ With Lafayette’s prestige and Mirabeau’s reputation as an orator at their respective apogees, Clarkson doubtless hoped for another emotional upsurge such as had produced the whirlwind abolition of serfdom and feudal rights on August 4. Indeed, slavery was eventually abolished in just this fashion in February 1794. It was more significant, however, that during the night of August 4 La Rochefoucauld had brought up the question of slavery without any success.²⁰ And, even earlier, members of the Constituent Assembly began to display the impatience with anticolonial speakers that was soon to become typical.²¹ Even so, as the Assembly prepared the Declaration of the Rights of Man, planters in Paris were sufficiently alarmed to form a pressure group to defend their interests. It became known as the Club Massiac.

On the basis of the fear expressed in colonial correspondence sent from Paris,

mass support: Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (New York, 1986); Seymour Drescher, “Two Variants of Anti-Slavery,” and Serge Daget, “A Model of the French Abolitionist Movement,” in Christine Bolt and Seymour Drescher, eds., *Antislavery, Religion and Reform* (Folkestone, 1980). Moreover, the French Revolution also accomplished many reforms, from civil rights for Jews to the abolition of the nobility, for which popular support was not evident in 1789.

¹⁶ Beatrice F. Hyslop, *French Nationalism in 1789 according to the General Cahiers* (New York, 1934), 142, 276–77; *Archives parlementaires de 1787 à 1880* [hereafter, AP], 7: 296–97.

¹⁷ *Journal de l’Assemblée Nationale*, 1 (1789): 8; [Jacques-Pierre Brissot], *Lettre à MM. les députés des trois ordres* (Paris, 1789), 30.

¹⁸ *Journal des États-Généraux*, 1 (1789): 260; *Journal de l’Assemblée Nationale*, 1 (1789): 244–48, 255–57; AP, 8: 165, 186–87.

¹⁹ Robert I. Wilberforce and Samuel Wilberforce, *Life of William Wilberforce*, 5 vols. (London, 1838), 1: 229–30.

²⁰ Jacques Thibau, *Le temps de Saint-Domingue: L’esclavage et la Révolution Française* (Paris, 1989), 147.

²¹ *Journal de l’Assemblée Nationale*, 1 (1789): 249, 257.

Yves Bénot has suggested that the antislavery cause perhaps missed a promising opportunity in mid-1789.²² Even seven months later, by which time the pro-colonial lobby had marshaled formidable forces against its opponents, the centrist deputy Regnauld of Saint-Jean-d'Angély feared that the Assembly might still be carried away by emotive eloquence. The slave trade was an issue on which most deputies, he thought, had feelings rather than an opinion. Others agreed.²³ By the beginning of 1790, when it was clear that the antislavery cause had lost much ground, Mirabeau, after personally taking soundings, apparently still reckoned that three hundred deputies would support abolition of the slave trade unconditionally and that another five hundred would do so if Britain agreed to abolish its slave trade.²⁴

Thomas Clarkson, who then believed the British trade would soon be ended, considered the French slave trade a more vulnerable target.²⁵ Smaller and less efficient than its British counterpart, it received annually 2 million livres in subsidies from the now bankrupt government. French slave merchants covertly used the services of British shipowners and brokers, and their export cargoes consisted largely of foreign goods. French planters also purchased many of their slaves from foreign contraband traders. Hence it could be claimed that banning the importation of slaves into France's colonies was not just a moral issue but was in the national interest and would hurt foreigners more than the French. (This tactic was used in Parliament to bring about the end of the British slave trade in 1806–1807.²⁶) The Constituent Assembly, however, refused even to hear such arguments. No abolitionists were elected to its Colonial Committee, and, on March 8, 1790, it passed a decree granting the colonies internal legislative autonomy. With a fastidious use of euphemism, it gave assurance that the mother country would respect “local customs,” protect “colonial property,” and would not “innovate in any branch of colonial trade.” The decree was hurriedly approved without discussion, and the antislavery supporters who endeavored to speak were shouted down. Most newspapers welcomed the outcome.²⁷

The colonial deputies subsequently boasted to their constituents that they had suggested almost every clause in the March 8 decree.²⁸ This ascendancy of the colonial faction resulted from a combination of ingredients. First, French mercantile interests and the Club Massiac, burying their differences over commercial regulation, proved especially adept at lobbying deputies, publishing propaganda, and piling up petitions and addresses from the port cities, which

²² Bénot, *Révolution française*, 107–08.

²³ *Journal de Versailles*, 125 (March 4, 1790); Hugh Gough, *The Newspaper Press in the French Revolution* (Chicago, 1988), 55; Thésée, “Amis des Noirs,” 63.

²⁴ Thésée, “Amis des Noirs,” 59.

²⁵ Thésée, “Amis des Noirs,” 35–36, 79–82. Abolition had been under consideration by the British Parliament since 1788, and a bill eventually passed the House of Commons, though not the Lords, in 1792.

²⁶ Roger Anstey, “A Reinterpretation of the Abolition of the British Slave Trade, 1806–1807,” *English Historical Review*, 87 (1972): 304–32.

²⁷ *AP*, 12: 68–73; Thésée, “Amis des Noirs,” 71.

²⁸ *Affiches américaines*, Feuille du Cap-François, 56 (July 14, 1790): 1.

claimed that the nation's prosperity was at stake.²⁹ Suspected of being in league with the British, the Amis des Noirs was clearly on the defensive by January 1790 in the battle for public opinion. Colonialist pressure forced Olympe de Gouges's play, *L'Esclavage des Nègres*, from the theater and then halted publication of Antoine Bonnemain's *Régénération des colonies*.³⁰ Although the Atlantic ports were not well integrated into the French economy, they (and consequently the slave societies they serviced) represented what was most modern and dynamic in the *ancien régime*. Serfdom and slavery were, therefore, not seen in the same light. The support of the wealthy seaports appeared crucial to the future of the revolution. Its safety, finances, and unity should come first, claimed Antoine-Pierre Barnave, the liberal democratic leader and spokesman of the Colonial Committee.³¹

To criticize the colonial status quo thus came to seem unpatriotic, and the March 8 decree actually made it a crime to incite unrest in the colonies. The stakes were raised this high because the debate was never focused narrowly on the slave trade alone. Even though the Amis des Noirs periodically asserted that the slaves were not yet "ready" for freedom, their writings made clear that abolition of the slave trade was to be just the first stage in the Société's attack on slavery.³² As a result, Barnave and the colonial deputies accused the antislavery lobby, unfairly, of being opposed to colonization in general.³³

Far more serious, the imperial tie also seemed endangered by the white settlers themselves and by the slaves. From early in the revolution, Saint Domingue's deputies claimed their colony to be an ally, not a dependent, of France, a position rooted in the island's past. Its articulation became especially strident when the revolution appeared to threaten the colonial system.³⁴ In the winter of 1789, the colonial deputies hinted openly that France needed its colonies more than the colonies needed France, and that if their wants were not met, secession might follow.³⁵ Substance was lent to these threats by the overthrow of the Old Regime administration in the West Indies and the

²⁹ The classic study is Gabriel Debien, *Les colons de Saint-Domingue et la Révolution française: Essai sur le Club Massiac* (Paris, 1953). The statement in Robin Blackburn, *The Overthrow of Colonial Slavery* (London, 1988), 178–79, that articles in the press warned that emancipation would lead to the distribution of property in France seems to be based on a misreading of Debien, *Les colons*, 185.

³⁰ Olympe de Gouges, *Réponse au champion Américain* (Paris, 1790); Thésée, "Amis des Noirs," 63; Antoine Bonnemain, *Régénération des colonies* (Paris, 1792), 3.

³¹ AP, 12: 68–69.

³² Marie-Jean-Antoine-Nicholas de Caritat, marquis de Condorcet, *Au corps électoral contre l'esclavage des noirs* (Paris, 1789), in *Oeuvres complètes de Condorcet*, 21 vols. (Brunswick, 1804), 16: 147–57; [Brissot], *Lettre à MM. les députés des trois ordres*; Henri Grégoire, *Mémoire en faveur des gens de couleur libres ou sang-mêlés de St.-Domingue* (Paris, 1789); *Patriote françois*, 119 (December 5, 1789), 121 (December 7, 1789).

³³ AP, 11: 38; 12: 68–69.

³⁴ For differing assessments of "creole autonomism," see Gabriel Debien, *Esprit colon et esprit d'autonomie* (Paris, 1954); Charles Frostin, "L'Histoire de l'esprit autonomiste colon à Saint-Domingue au XVII^e et XVIII^e siècles" (thèse de doctorat d'état, Paris I, 1972); David Geggus, *Slavery, War and Revolution* (Oxford, 1982), 16–18, 31–32, 35–37, 46–78.

³⁵ AP, 8: 138; 10: 266–67; 11: 38; 12: 381–87; Pierre-Joseph Laborie, *Reflexions sommaires adressées à la France et à la colonie de Saint-Domingue* (Paris, 1789), 13–14; Garrett, *French Colonial Question*, 12. The deputy de Cocherel's claim to "envision with equanimity the end of the slave trade," mentioned in Boulle, "Defense of Slavery," 232, was really an artful reminder that planters could buy more cheaply from British than French traders.

colonists' establishment of their own assemblies, developments that finally forced the Constituent Assembly to pay attention to colonial matters.

Autonomist movements in the Caribbean were further spurred by the fear of slave rebellion.³⁶ Reports reached Paris in December 1789 of a brief slave revolt in Martinique and of unspecified disorders on plantations in Saint Domingue, supposedly provoked by news from France.³⁷ Such reports were greatly exaggerated and, coinciding as they did with Abbé Henri Grégoire's ill-timed call for a "general insurrection" against tyranny,³⁸ they fed merchants' and planters' efforts to depict abolitionists as short-sighted, possibly criminal, fanatics. The mood of the Constituent Assembly changed considerably.³⁹ Barnave was thus able to introduce the March 8 decree as an indispensable measure for reassuring the colonists and preserving the colonies.

In this manner, the slave trade and slavery became taboo subjects that the Constituent and Legislative assemblies simply refused to discuss. The demand for reform proved far weaker than even the *cahiers* had suggested.⁴⁰ In May 1791, the Assembly passed a constitutional decree that explicitly guaranteed the slave regime against metropolitan interference. Robespierre objected violently, uttering his famous words, "Perish the colonies!"⁴¹ He also observed, less memorably, in the same speech, "The conservation of your colonies is an important matter." The cause of his outburst was not the institution of slavery but concern for the domestic impact of a proposal to use the word "slave" in the decree (instead of "unfree person"). Except for future dramatic developments in the Caribbean, probably little more would have been heard of these issues. The slave trade was not abolished during the revolution; it reached its peak during the years 1789–1791.⁴²

The Constituent Assembly sought to avoid the question of racial equality as well, but in a more devious manner and with frank embarrassment. In contrast to the antislavery debate, the cause of the *gens de couleur libres*—the free colored middle class of the colonies—was all but unknown in France. Numbering forty thousand or so in the Caribbean, they were a diverse group. Most were of mixed

³⁶ From August onward, colonists in Paris were writing home that the Amis des Noirs might try to foment a slave revolt. Colonial radicals in Saint Domingue used such fears to mobilize support and undermine the administration: Peiner to La Luzerne, October 24, and November 12, 1789, Archives Nationales, Paris (hereafter, AN), cols. C9/162 and C9/163; Louis François Roger Armand Gatereau, *Histoire des troubles de Saint-Domingue* (Paris, 1792), 12–14.

³⁷ *Patriote français*, 117 (December 3, 1789), 133 (December 24, 1789), 144 (December 30, 1789); AP, 11: 38, 710.

³⁸ "Yes, the cry of liberty rings out in two Worlds; it needs only an Othello, a Padrejean, to awaken in the soul of the Negroes a sense of their inalienable rights"; Grégoire, *Mémoire en faveur des gens de couleur*, 35–36. Padrejean had led a slave revolt in seventeenth-century Saint Domingue.

³⁹ Jean Philippe Garran Coulon, *Rapport sur les troubles de Saint-Domingue*, 4 vols. (Paris, 1797–99), 1: 127.

⁴⁰ Only seven deputies had opposed the March 8 decree. Plans for slave emancipation were put to the assemblies by two obscure deputies named Vieffville des Essarts (in May 1791) and Blanc-Gilli (in December 1791). Neither was discussed.

⁴¹ AP, 26: 60.

⁴² Brissot's sense of urgency regarding the trade was not misplaced. In 1789, eighty-three slave ships sailed from Nantes and Bordeaux alone, and in 1790 more than forty thousand Africans were sold just in Saint Domingue; Jean Mettas, *Répertoire des expéditions négrières*, eds. Serge Daget and Michelle Daget, 2 vols. (Paris, 1978, 1984); *Affiches américaines* (1790), port statistics.

racial descent, and they included wealthy, well-educated, slaveholding families in addition to recently freed slaves. All were subject to humiliating, racist restrictions. According to colonists and colonial officials, the stability of the slave regime demanded that all descendants of slaves be consigned in perpetuity to a dishonored intermediary class.⁴³ In 1778, mixed marriages and immigration by non-whites were banned even in France, where there lived several thousand non-whites, chiefly in Paris and the Atlantic ports. During the twenty years prior to the revolution, however, government officials began cautiously to discuss ameliorating the position of those in the colonies who were wealthiest and furthest removed from black ancestry. The *Encyclopédie* included a favorable article on *mulâtres*, which was echoed in early pamphlets of Brissot, the *cahier* of the Paris Third Estate, and some early altercations in the National Assembly. Even so, during the first years of the revolution, the French public still sometimes confused slaves and free coloreds.⁴⁴

NEVERTHELESS, IT WAS THE RACE QUESTION that was to prove the most prominent of colonial issues during the Constituent Assembly. Free colored activists in both Paris and Saint Domingue brought their cause to public notice in the fall of 1789, and, taken up by Brissot and Grégoire, it thereafter became the main campaign of the Amis des Noirs. Much remains unclear about the genesis of this alliance and the evolution of free colored demands.⁴⁵ Particularly uncertain is the importance attached in the debate to racial intermixture (specifically, to white ancestry). Since blacks were only a minority of free non-whites, in contemporary parlance they were often subsumed within the ambiguous term "people of color." However, phenotypical distinctions were extremely important in the colonies, where they formed the basis of a racial hierarchy and tended in addition to coincide with differences in wealth, literacy, and genealogical distance from slavery.⁴⁶ Juxtaposing questions of race and class, these internal divisions within the free colored sector merit much more research.⁴⁷

The early political activities of these non-whites reflected a clash between

⁴³ Auguste Lebeau, *De la condition des gens de couleur libres sous l'Ancien Régime* (Poitiers, 1903); Geggus, *Slavery, War and Revolution*, 18–23; John Garrigus, "Between Servitude and Citizenship: Free Coloreds in Pre-Revolutionary Saint Domingue" (Ph.D. dissertation, Johns Hopkins University, 1988).

⁴⁴ Yvan Debbaesch, *Couleur et liberté: Le jeu du critère ethnique dans un ordre juridique esclavagiste* (Paris, 1967), 141–42, 157–59, and *passim*.

⁴⁵ Debbaesch, *Couleur*, 144–66, is the most detailed study. The account in Debien, *Les colons*, 153–65, probably overrates the influence of the Amis des Noirs and Julien Raimond but seems the more accurate as regards chronology. Each relied on different copies of the Club Massiac's papers.

⁴⁶ Médéric L. E. Moreau de Saint-Méry, *Description topographique, . . . de Saint-Domingue*, eds. B. Maurel and E. Taillemite, 3 vols. (1797; Paris, 1958), 1: 83–111. On literacy, see David Geggus, "Slave and Free Colored Women in Saint Domingue," in Darlene Clark Hine and D. Barry Gaspar, eds., *Black Women in Slavery*, forthcoming.

⁴⁷ Highlighted by some right-wing historians, they are generally ignored by those on the left. During the revolutionary era in the Caribbean, free blacks often aligned themselves either with whites or slaves rather than other free coloreds; Marcot to Brissot, April 30, 1791, AN, Dxxv 78/771; Geggus, *Slavery, War and Revolution*, 325–26, 330; David Geggus, "The French and Haitian Revolutions and Resistance to Slavery in the Americas," *Revue française d'histoire d'Outre-Mer*, 282–83 (1989): 109–11.

traditional colonial values and the new ideology of equal rights. The first pamphlet written by a free colored appears to be exclusively a plea for fellow "mixed bloods."⁴⁸ Its author wanted colonial society divided along color lines, with the mulattoes and whites grouped together. Mulatto slaves were to be freed at birth. He referred to blacks as destined for slavery. In late August 1789, the wealthy quadroon⁴⁹ Julien Raimond sought political rights only for fellow quadroons, a policy of limited assimilation he had been discreetly urging on the government for some years.⁵⁰ Following the Declaration of the Rights of Man, however, some thirty to fifty "citizens of color" in Paris formed the Société des Colons Américains and drew up a much more radical *cahier* in early September. It called for full equality for all free non-whites and freedom for mulatto slaves (provisions that respectively negated and affirmed the racist assumptions of colonial society). Raimond and another quadroon, Vincent Ogé,⁵¹ joined the Colons Américains shortly thereafter. French historians Gabriel Debien and Yvan Debbasch treat the Société's *cahier* with some skepticism, deducing a willingness to compromise on its demands from the fact that Raimond, Ogé, and the Société all sought at this period to by-pass the Constituent Assembly and negotiate directly with the Club Massiac.⁵² When, in October and November, the Société sought representation in the Assembly, their addresses spoke only of "citizen landowners of color . . . born free," "mulattoes, quadroons, etc." In the writings of Raimond, too, Debbasch observes, there remained a discordance between an egalitarian program and a discourse tainted by "sub-racism" that minimized the numbers of free blacks.⁵³

Debbasch and Debien do not agree on exactly when free blacks were admitted to the Société des Colons Américains (whose membership grew to eighty), but their numbers were evidently few.⁵⁴ In November, an open rift appeared when an anonymous group of free blacks published an address that vaunted their racial purity over the "bastardized" origins of the citizens of color and demanded separate representation for themselves, should the mulattoes gain seats in the Assembly.⁵⁵ Behind this maneuver stood the deputies for Saint Domingue, who

⁴⁸ *Précis des gémissements des sang-mêlés dans les colonies françaises, par J. M. C. Américain, Sang-mêlé* (Paris, 1789), 7, 9, 10, 13. It does call in passing for the implementation of the defunct Code Noir of 1685, which had accorded equal status to all freemen, irrespective of color, but the author's other comments belie this suggestion.

⁴⁹ Mulattoes were persons of black/white parentage; quadroons, of white/mulatto parentage.

⁵⁰ Césaire, *Toussaint*, 86; and Debbasch, *Couleur*, 151, showed a radical change in Raimond's proposals between August 26 and 28, but their accounts are clearly confused. To judge from Debien's earlier study (*Les colons*, 158), they seem to have been misled by a false rumor spread by a colonist. On Raimond's background, see John Garrigus, "The Free Colored Elite of Pre-Revolutionary Saint Domingue: The Case of Julien Raimond," in D. Barry Gaspar and Gad Heuman, eds., *Brown Power in the Caribbean*, forthcoming.

⁵¹ Not the brilliant lawyer of legend, Ogé was an unsuccessful merchant from Saint Domingue then pursuing a lawsuit in Paris. When later interrogated in the colony, he commented that "he had never been friendly with any free blacks and . . . he scarcely knew any"; interrogation of Ogé, AN, Dxxv, 58/574.

⁵² Debbasch, *Couleur*, 146–54; Debien, *Les colons*, 164.

⁵³ AP, 9: 476–78; 10: 329–33; Debbasch, *Couleur*, 153–54.

⁵⁴ Debien, *Les colons*, 164; Debbasch, *Couleur*, 149, 151.

⁵⁵ AP, 10: 329–34, annex to session of November 28. Apparently not presented to the Constituent, the address was published in the next day's *Moniteur universel*. Two weeks before, the

claimed fully to represent the colony already and who wished to ridicule their rivals' demands. But the document probably was not a forgery, as has been claimed.⁵⁶ Its authenticity was not challenged at the time, and Brissot in the *Patriote françois* suddenly stopped using "free black" as a synonym for "free colored."⁵⁷ The breach was nevertheless of short duration. Although a sense of separate identity had suggested separate strategies to these different factions, they were visibly cooperating by early 1790, brought together by the new ideology of equal rights and the increasing intransigence of the colonists.⁵⁸

It is nonetheless striking that the free coloreds' white allies continued to focus on white ancestry as an important attribute of the free coloreds. Although Grégoire did support the cause of free blacks, he almost always used the terms "mixed bloods," "mulattoes," and "people of color" in his advocacy, unlike Brissot, who frequently referred to "free blacks."⁵⁹ The concern with free coloreds as "descendants of Europeans" is explicit in the writings of the abbé de Cournand, another Ami des Noirs and early champion of colonial non-whites. He called for full equality for colored landowners who were two generations removed from slavery and, like Grégoire, for freedom at birth for slaves of mixed racial descent.⁶⁰ Even in 1791, the Amis des Noirs' addresses, while asserting the equality of all free non-whites, described them as "mulatto Frenchmen." A similar tendency is evident in the addresses of the Jacobin Clubs.⁶¹

Their choice of language was surely not accidental. Even though there was probably genuine misunderstanding about the true number of free blacks in the colonies,⁶² their activities in Paris meant they could not be ignored. Some reformers perhaps were willing to settle for a shift in the line of demarcation rather than the abolition of racial discrimination. Most likely, they thought that a discourse that stressed the mixed heritage of the *gens de couleur* had the best

Société had denied any prejudice toward free blacks and proposed to defend itself in the press against the charge: *procès-verbal*, November 14, 1789, AN, Dxxv 111.

⁵⁶ In A. Brette, "Les gens de couleur libres et leurs députés en 1789," *La Révolution Française*, 29 (1895): 399; and Garrett, *French Colonial Question*, 4.

⁵⁷ The change occurred between numbers 110 (November 26, 1789) and 117 (December 3, 1789). Compare Grégoire, *Mémoire en faveur*, 25; AP, 10: 333.

⁵⁸ In the summer of 1789, a minority of colonists in Saint Domingue and Paris had shown themselves favorable in some degree to the free coloreds' aspirations, and this doubtless had encouraged hopes for an accommodation; Claude Milscent, *Du régime colonial* (Paris, 1792), 5; Garran Coulon, *Rapport*, 1: 106, 2: 3–8; Debien, *Les colons*, 156–69.

⁵⁹ Compare Brissot's *Lettre à MM. les députés* and *Patriote françois*, 65 (October 9, 1789) onward; and Grégoire's *Mémoire en faveur* and *Lettre aux philanthropes sur . . . les réclamations des gens de couleur* (Paris, 1790). Only after some free blacks were enfranchised by the decree of May 15, 1789, did Grégoire begin using the term frequently: *Lettre aux citoyens de couleur, et nègres libres* (Paris, 1791), particularly p. 2, where he switches from the vocative case to refer to his past efforts on behalf of "mixed-bloods." The contrast is all the more striking with his posthumous memoirs, in which he uses the phrases "free blacks and mulattoes" and "free blacks and mixed bloods"; Hippolyte Carnot, ed., *Mémoires de Grégoire*, 2 vols. (Paris, 1837), 1: 391–92.

⁶⁰ Antoine de Cournand, *Requête présentée à Nosseigneurs de l'Assemblée Nationale en faveur des gens de couleur de l'île de Saint Domingue* [Paris, 1789?], 2, 10, 11.

⁶¹ Etienne Clavière, *Adresse de la Société des Amis des Noirs à l'Assemblée Nationale*, 2d edn. (Paris, 1791), xii, 48, 56, 69–70, 111; *Lettres des diverses Sociétés des Amis de la Constitution qui réclament les droits de Citoyen actif en faveur des hommes de couleur des Colonies* (Paris, 1791), 8, 9, 16, 18.

⁶² In Saint Domingue, they constituted about one-third of free coloreds.

chance of overcoming opposition and persuading the French to think of colonial non-whites as their fellow citizens. With the position of Jews and non-whites in France left in legal limbo until September 1791, it was the question of free coloreds in the colonies that first tested the racial boundaries of the French Revolution.

Here again, as on the question of the slave trade, the Constituent Assembly ended with the status quo intact, and for much the same reasons. The issues were more subtle, however, and there developed a greater diversity of opinion. The campaign began well on October 22, 1789, when at the bar of the Assembly the delegates of the Société des Colons Américains received vague but encouraging words from the president of the session. In November, their demand for free colored representation in the Assembly was accepted by a narrow vote in its Credentials Committee, chaired by Grégoire. Yet, because of extraordinary obstruction, the committee's report was never presented, and when Grégoire tried to speak he was shouted down.⁶³ The proposed deputies could not prove that they had any representative standing, and the Constituent Assembly had already opposed such special interest representation in the case of the mercantile lobby. However, it was not these arguments but raw political power that denied the free coloreds a voice, first in the National Assembly, then in the colonial assemblies recognized by the decree of March 8.

Resting squarely on the Declaration of the Rights of Man, the civil rights of tax-paying, property-owning, non-whites seemed a much safer issue to the Amis des Noirs than did the slave trade. It involved none of the conflict between rights to liberty and property found in the case of human bondage. Yet, despite their insistence that the free colored question was separate from that of slavery, the Amis did not succeed in divorcing the two issues. Brissot incautiously wrote that free colored citizenship would prepare the way for eventual emancipation of the slaves; this link remained a constant theme in antislavery literature.⁶⁴ It also seemed that free colored deputies would become allies in the fight against the slave trade.⁶⁵ And free colored attitudes toward slavery (an unresearched topic) could be perplexingly ambivalent.

It is broadly true that non-white slaveowners generally acted according to their class interests during the struggles of the 1790s. Yet, in an early address to the Constituent Assembly, the Colons Américains went out of its way to condemn the institution of slavery. Astonishingly, Vincent Ogé made a speech to a stonily silent Club Massiac in September 1789, alluding to preparing for the end of slavery, that left him a marked man.⁶⁶ And, as already seen, both free coloreds and white abolitionists linked free colored citizenship with the emancipation of

⁶³ AP, 9: 476–78; Grégoire, *Lettre aux philanthropes*, 5–6; *Patriote français*, 119 (December 5, 1789).

⁶⁴ *Patriote français*, 65 (October 9, 1789); 77 (October 23, 1789); 121 (December 7, 1789); Grégoire, *Lettre aux citoyens*, 13; Julien Raymond, *Réflexions sur les véritables causes des troubles* (Paris, 1793), 21–29; Bénot, *Révolution*, 79–80, 128.

⁶⁵ Thésée, "Autour des Amis," 17.

⁶⁶ AP, 10: 330; Vincent Ogé, *Motion faite par M. Vincent Ogé, jeune à l'Assemblée des Colons* (Paris, 1789), 5; Debien, *Les colons*, 160. Ogé was kept under close surveillance thereafter by the Club Massiac. He is usually remembered for rigidly excluding slaves from the rebellion he led in Saint Domingue a year later.

slaves of mixed racial descent. Finally, although antislavery writers were at pains to argue that elevating the status of free non-whites would strengthen the slave regime (and act as a check on white secessionism), both Grégoire and Brissot also suggested that, if driven to desperation, free coloreds would join with the slaves (or secessionist whites) in open revolt.⁶⁷

The colonists also agreed that the race and slavery questions could not be kept separate. In the Caribbean, colonists and officials had traditionally regarded racial prejudice and discrimination as necessary bulwarks of slavery. They supposedly helped convince blacks of their inherent inferiority and prevented the relatives of slaves gaining access to public office. This argument seldom appeared early in the revolution, however. Legerdemain was used instead. For example, in February 1790, the Paris Commune warmly received a deputation of non-whites but, after hearing arguments from both sides on the colonial question, passed on to other business.⁶⁸ Similarly, Barnave's Colonial Committee wanted the decision on free colored voting rights to be made by the colonial assemblies, but it dared not admit this. In defining the franchise, therefore, in the decree of March 8 and its accompanying instructions, it made deliberately ambiguous reference to "citizens" or "persons," without mention of phenotype.⁶⁹ When a debate was finally forced and Grégoire sought clarification, the session was hastily closed, with Barnave and others giving off-the-record assurance that the rights of free coloreds were not infringed.⁷⁰ In the Caribbean, both white planters and royal administrators interpreted the decree according to colonial norms. Their view became the "official" one, and on October 12 Barnave added to another colonial decree (forced through without debate) a promise that the National Assembly would not legislate on the "status of persons" in the colonies.⁷¹

There are a few tantalizing signs that things could have turned out differently. Colonial opinion was by no means monolithic, and the race question interested merchants notably less than planters. Racial conflict assumed different proportions in different colonies, and even within the Club Massiac and in parts of Saint Domingue there was movement in 1789 toward cooperation between whites and free coloreds.⁷² Hence there was some basis for accommodation. The nature and causes of these divergent approaches merit further investigation, but most likely they were not very influential.⁷³ In any event, a polarization of whites and free coloreds rapidly took place as the race and slavery issues became entwined. While free coloreds were being lynched in Saint Domingue and Martinique, the Constituent Assembly rallied solidly behind Barnave's efforts not to alienate white colonial opinion. The Assembly's concern to reassure colonists and to avert any movement toward secession was particularly explicit in the tormented

⁶⁷ *Patriote français*, 117 (December 3, 1789); Grégoire, *Mémoire en faveur*, 33, 51; Grégoire, *Lettre aux philanthropes*, 14; *Moniteur universel* (February 16, 1790).

⁶⁸ *Moniteur universel* (February 16, 1790).

⁶⁹ Debien, *Les colons*, 190–96.

⁷⁰ AP, 12: 381–87, 25: 738.

⁷¹ Grégoire, *Lettre aux philanthropes*, 1–2.

⁷² See above, note 58.

⁷³ Gatereau, *Histoire*, 19–21; Debbasch, *Couleur*, 169–72.

colonial debate of May 1791, which led to the constitutional guarantee of inviolability for the slave regime but also to a reversal on the race issue.⁷⁴

By May 1791, pro-colonial speakers faced a new degree of opposition on the floor of the Assembly, in its public galleries, and in the streets outside. During the previous six months, the political situation had changed noticeably, partly because of a short-lived rebellion in northern Saint Domingue led by an exasperated Vincent Ogé.⁷⁵ News of Ogé's being tortured and broken on the wheel reached France early in the spring and helped bring home to the Constituent Assembly the implications of colonial self-government. Fighting between urban and rural whites in the Caribbean also encouraged a more interventionist policy toward the colonies. The Assembly dispatched troops and drew up plans to send out civil commissioners with extensive powers. Many deputies now saw concessions to the free coloreds not only as a means of atonement for the dishonest neglect of the past but as a way of strengthening France's loose grip on its colonial populations.⁷⁶ The port of Bordeaux announced its support for the *gens de couleur*.⁷⁷

The political climate was also changing in France. Growing antipathy toward the aristocracy and dissatisfaction with the limitation of the franchise probably put in a more sympathetic light the free coloreds' case against the "aristocracy of the skin." At the very least, free colored rights provided a vehicle with which disgruntled Jacobins could attack the political status quo.⁷⁸ Michael Kennedy's study of the Jacobin Clubs shows that support for the free coloreds grew in early 1791 more in response to Grégoire's advocacy than to events in Saint Domingue. Yet the provincial Jacobins' petitioning campaign was sparked directly by the Ogé affair and was apparently the work of Claude Milscent in Angers, a maverick liberal planter.⁷⁹ Some historians see in this period an upswelling of popular support for the free coloreds' cause.⁸⁰ However, the extent of public interest in colonial affairs during the revolution remains problematical. For example, the support of the provincial Jacobin Clubs and the Bordeaux merchants appears to have been somewhat fragile.⁸¹

After four days of heated debate on the rights of free coloreds and the

⁷⁴ AP, 25: 636–40, 743, 755.

⁷⁵ The rebellion remained a local affair, true to Debbasch's characterization of free colored politics as fractured and isolated. The parish where it occurred, however, appears to have been in early contact with the movement in Paris: petition of Grande Rivière free coloreds, November 11, 1789, Brissot Papers, Marcel Châtillon collection, Paris. Early in 1790, moreover, Ogé's elder brother had been killed leading free colored petitioners in the neighboring West Province: Verrier to Legrand, February 25, 1790, AN, Dxxv 72/718; interrogation of Ogé, AN, Dxxv, 58/574.

⁷⁶ Blackburn, *Overthrow*, 187, 210.

⁷⁷ AP, 25: 737, 26: 357–60; *Lettres importantes relatives à la question des citoyens de couleur* (Paris, 1791), 1–3; *Lettres des diverses sociétés*, 6–7.

⁷⁸ Blackburn, *Overthrow*, 185–88.

⁷⁹ Michael Kennedy, *The Jacobin Clubs in the French Revolution* (Princeton, N.J., 1982), 204–09; see Claude Milscent, *Sur les troubles de Saint-Domingue* (Paris, 1792); and *Lettres des diverses Sociétés*. Like Ogé, Milscent was from the central mountains of the North Province.

⁸⁰ Cyril L. R. James, *The Black Jacobins*, rev. edn. (New York, 1973), 67–68, 75–76; Bénot, *Révolution*, 86–87, 200–01.

⁸¹ Kennedy, *Jacobin Clubs*, 209; AP, 30: 123.

“necessity” of racial discrimination,⁸² a compromise decree was passed on May 15, 1791. It enfranchised only non-whites born of free parents, not freedmen.⁸³ It thus kept in existence an intermediary class, as the colonists wished, but one based on legal standing, not phenotype. Furthermore—a point often overlooked—it accorded only voting rights without touching discrimination in any other area. Still, the white colonists regarded the decree as a dangerous breach in the colonial system. Colonial deputies walked out of the Constituent Assembly. Planters in Saint Domingue spoke ever more loudly of secession, and the colony’s governor wrote home that he could not enforce the decree.⁸⁴ In Paris, a climate of political reaction set in following the flight of the king. Interaction between events in the colonies and metropole accelerated, but colonial policy continued to be fatally out of step with developments in the Caribbean. In a remarkable *volte-face*, Barnave managed to have the decree rescinded in the closing days of the Constituent Assembly, even as Jews, actors, and—a conciliatory gesture—non-whites in France had their civil rights recognized.⁸⁵

By then, frustrated free coloreds in Saint Domingue had already taken up arms and forced acceptance of their demands from recalcitrant planters. They were able to do this because in August 1791 there broke out in the colony’s northern plain the largest slave revolt in the history of the Americas. Its origins are obscure, but the strengthening of counterrevolutionary currents in Europe and rumors associated with the suppression of the May 15 law seem to have been influential.⁸⁶ Also, the prospect of secession probably aroused no enthusiasm in the slave quarters. The planters for their part commonly blamed the insurrection on the interference of abolitionists, free coloreds, or counterrevolutionaries. Whatever its causes, the Saint Domingue slave revolt decisively changed the colonial situation, first pushing white planters to take overt steps toward secession (1791), then bringing French legislators to concede full racial equality (1792) and eventually slave emancipation (1794).⁸⁷

IN A SENSE, THERE ARE GOOD GROUNDS FOR NEGLECTING colonial affairs in histories of the Constituent Assembly. In October 1791, slavery, the slave trade, and racial

⁸² Opponents of the free coloreds did not attack them in racist terms in the formal debate but rather argued for the utility of prejudice as a stabilizing force in slave society. The role of racist ideas is difficult to pin down, historians disagreeing as to the prevalence of “biological racism” in this period. This is partly because the then-influential theories of Buffon can be interpreted as either biological or environmental.

⁸³ The number of beneficiaries would have been further limited by the probable need for the parents to have been legally married in order to prove the father’s status. Even so, the oft-repeated statement that in Saint Domingue the decree affected only four hundred persons seems scarcely credible.

⁸⁴ *AP*, 30: 118–19; Geggus, *Slavery, War and Revolution*, 51–52.

⁸⁵ Slaveholding within France was also abolished at this time, but, like the status of non-whites in France, this had never been an issue. Even the colonial deputy Cocherel had suggested in November 1789 that all blacks should be free, so long as they stayed in France; *AP*, 10: 266.

⁸⁶ The insurgents claimed to be defenders of the king, who was said to have passed an emancipation decree. This was to be a common feature of American slave rebellions for forty years; Geggus, “French and Haitian Revolutions,” 119–21.

⁸⁷ Geggus, *Slavery, War and Revolution*, 52–54.

discrimination in the colonies all remained juridically unchanged. Although the Assembly had admitted seventeen colonial representatives, it had recognized no deputies for the non-whites, who made up more than 90 percent of the colonies' inhabitants. French overseas possessions had been placed outside the new constitution, and the colonies were left self-governing in internal matters, free to choose which aspects of the national regeneration they would adopt.

Nonetheless, Caribbean politics could not be kept out of the Constituent Assembly. By early 1791, the creation of civil commissioners and the dispatch of troops had begun to undermine the colonial autonomy granted the preceding year. Deputies had to grapple with issues that painfully opposed political principles and national prosperity. On the racial question, they reversed course several times and produced in May one of the great debates of the early revolution. These events alone should gain for colonial matters a more prominent place in the historiography of revolutionary France.

Also relevant to the question of the centrality of colonial affairs to the French Revolution is the relative importance of overseas and metropolitan influences in creating policy and shaping the colonial revolution. To what extent did colonial policy evolve out of developments in France, and to what degree was it generated by events in the colonies that may have been only tangentially related to France's revolution? Where in this colonial question should one locate core and periphery? Here is perhaps the greatest challenge to the historian of the colonial revolution. The work of the distinguished West Indian Marxists C. L. R. James and, in particular, Aimé Césaire has done much to establish the colonial revolution as an autonomous force that helped to radicalize the French Revolution.⁸⁸ Similar arguments have been presented by writers of the Haitian *noiriste* school and also by some European scholars on the right.⁸⁹ Creole autonomism, free colored activism, and slave resistance certainly had complex pre-histories independent of the revolution in France, and, as noted above, the Constituent's colonial legislation came largely as a response to events in the Caribbean.

The recent studies by Bénot and Robin Blackburn also support this viewpoint and achieve a new degree of sophistication in integrating metropolitan and colonial developments. But they additionally make a strong case, also within a Marxist framework, for giving renewed emphasis to idealist influences inside the French Revolution. Antislavery and anti-racism were politically weak forces, Bénot and Blackburn agree, but, together with the growth of French radicalism, they helped form the intellectual and emotional climate in which colonial policy was made, and they created political options that could be taken up as political calculations changed.⁹⁰

The evidence that Bénot, Blackburn, and James put forward of a growing

⁸⁸ James, *Black Jacobins*, *passim*; Césaire, *Toussaint*, 21–22, 159, 308–09.

⁸⁹ See, for example, Emmanuel C. Paul, *Questions d'histoire* (Port-au-Prince, 1956); Saint-Victor Jean-Baptiste, *Haiti: Sa lutte pour l'émancipation* (Paris, 1957); Erwin Rüsçh, *Die Revolution von Saint Domingue* (Hamburg, 1930); J. Saintoyant, *La colonisation française pendant la Révolution*, 2 vols. (Paris, 1930).

⁹⁰ Bénot, *Révolution*, 7–9, 18–20; Blackburn, *Overthrow*, 185–90, 195, 222–25, 230.

popular solidarity in France with colonial non-whites is suggestive but limited, and there seems some contradiction in Bénot's depiction of antislavery as possessing mass support yet remaining the cause of an enlightened handful of individuals.⁹¹ Nevertheless, in exploring the popular press, he has convincingly reconstructed the spread of antislavery sentiment beyond the elite Amis des Noirs and highlighted the roles of several neglected individuals whose interest in the colonies cannot be reduced to a pragmatic reaction to overseas events, though it did not always pre-date the revolution. Easily the most important of these was Léger-Félicité Sonthonax, the commissioner who unilaterally abolished slavery in Saint Domingue in 1793 and so precipitated the emancipation decree of 16 Pluviôse. Although his actions are best understood as a desperate response to a wartime emergency, they take on a new appearance when one realizes that as early as September 1790 Sonthonax was predicting the end of slavery, writing that "the time is not far off when a frizzy-haired African, with no recommendation beyond his good sense and virtue, will come and participate as a legislator in the bosom of our national assemblies."⁹²

Perhaps the most important point that one can make regarding the force of antislavery ideas, however, is to note that the moral case against slavery and institutionalized racism was often conceded by their leading defenders. Both were unjust and irrational, they admitted, but unfortunately necessary for French prosperity and the preservation of order in the colonies.⁹³ It is a measure of antislavery's success that its opponents were often reduced to a defense based on pragmatism that later proved vulnerable to recalculations of self-interest.

Finally, it is important not to ignore the way in which the decrees of March 8 and October 12, 1790, and May 15 and September 24, 1791, reflected the changing political climate in Paris: the triumph of bourgeois liberalism, the subsequent radical challenge, and the political reaction of the summer of 1791. Jaurès and James have argued that the about-face on the race question facilitated the post-Varennes reaction,⁹⁴ but on chronological grounds alone this would seem unlikely. All in all, while it is clearly fruitless to try to assign primacy to either French or overseas influences in creating colonial policy, the history of that policy cannot be treated in isolation from the development of the revolution in France.

Defining the French Revolution's impact on the colonies is a more difficult task. Despite an abundant historiography, current research does not allow

⁹¹ Bénot, *Révolution*, 75, 86–87, 200–04, 217; Blackburn, *Overthrow*, 223; James, *Black Jacobins*, 75–77, 120, 139.

⁹² Bénot, *Révolution*, 130; Robert Louis Stein, *Léger-Félicité Sonthonax: The Lost Sentinel of the Republic* (Rutherford, N.Y., 1985), 21. Exactly three years later in Saint Domingue, Sonthonax had African-born Jean-Baptiste Mars Belley elected deputy and sent to Paris to plead for general emancipation (see cover illustration). Ironically, he was one of the very few former slaveowners to sit in the Convention: AN, Séction d'Outre-Mer, Aix-en-Provence, Notariat, Saint-Domingue, reg. 1012, contract dated June 19, 1787.

⁹³ AP, 12: 68–73, 25: 749, 755, 31: 281–82, 294; Thibau, *Le temps*, 252; Bénot, *Révolution*, 37–39; Boule, "Defense," 232, 240.

⁹⁴ Jaurès, *Histoire socialiste*, 1: 574; James, *Black Jacobins*, 80–81. The king's attempt to escape from Paris in June is known as the Flight to Varennes. To appease the alienated monarch, the Assembly thereafter clamped down on radicals and introduced several conservative reforms.

precise formulations, as Bénot observed,⁹⁵ and his and Blackburn's excellent studies are notably stronger on metropolitan than colonial events. This is not the place to review the whole colonial revolution, but a few points can be made. Although Césaire's stress on the diversity of the revolutionary experience in the colonies is well founded, it does not entirely undercut those who argue for the French Revolution's importance in shaping the colonial revolution. Many similarities existed between events in the different colonies, and the central issues were generally the same, even if differences in demographic balance and geographic size produced differing outcomes.

The question is fundamentally one of weighing the ideological influence of the French Revolution against its political influence. Did the revolution create new social and political aspirations in the colonies or merely weaken the institutions that traditionally had held them in check? "Merely" is perhaps an overly dismissive term to describe the disruption of the colonial administration and military that took place in the period 1789–1791. There is certainly, I would argue, a vast difference in scale between developments before and after 1789 in white autonomism, free colored activism, and slave resistance.⁹⁶ Yet the specificity of the colonial revolution remains difficult to define, especially insofar as France and its empire were part of a broader Atlantic revolution.

French Caribbean whites behaved and spoke much like their compatriots in France, where the great majority had been born. The role of the principal law courts, the hatred of "administrative despotism," the adoption of the tricolor cockade, the creation of municipalities and political clubs, the pursuit of representative government, all were similar in mother country and colonies, even if the seigneurial pretensions of some planter revolutionaries made them look as much like aristocrats in Robert R. Palmer's broad definition as like the Patriots they usually claimed to be.⁹⁷ Anne Pérotin-Dumon's work on the Jacobins of the Windward Isles brings out their mimetism and fixation on the revolution in France. However, she also underlined the centrality of the race and slavery issues for these "Patriots in the Tropics" and showed how colonial values tempered their revolutionary zeal. Eighteenth-century attitudes to race require much more investigation, but, even though the difference in social setting was indeed enormous, the gap between France and its colonies was not necessarily so great. Just as metropolitan radicals were slow to take up the race and slavery issues, those in the West Indies were generally obliged to reconcile themselves, at least superficially, to the reforms of 1792 and 1794.⁹⁸

For many others, however, the end of white supremacy and slavery provided grounds for abandoning "la France régénérée." The revolution thus reinforced

⁹⁵ Bénot, *Révolution*, 138.

⁹⁶ On these, see above, note 22; Debbasch, *Couleur*; David Geggus, *Slave Resistance Studies and the Saint Domingue Slave Revolt* (Miami, Fla., 1983); and Geggus, "Marronage, Voodoo, and the Saint Domingue Slave Revolt," in P. Boucher, ed., *French Colonial Historical Society: Proceedings of the 15th Annual Conference*, forthcoming.

⁹⁷ Geggus, *Slavery, War and Revolution*, 33–37; Palmer, *Age of the Democratic Revolution*.

⁹⁸ Anne Pérotin-Dumon, "Les Jacobins des Antilles ou l'esprit de liberté dans les Îles-du-Vent," *Revue d'Histoire Moderne et Contemporaine*, 35 (1988): 275, 284–90, 302–04; Geggus, *Slavery, War and Revolution*, 43–44.

latent desires for independence and freedom from restrictions on foreign trade, the other area in which colonial and metropolitan interests clashed head-on. Even so, it is remarkable that, while in the Caribbean the trade laws were openly flouted during the revolution, the planter lobby in France scarcely raised the commercial question at all, so anxious was it to secure the port city merchants as allies.

As for colonial secessionism, I have argued elsewhere that it was a weaker movement than appearances suggested and that, as in France, counterrevolutionary forces played an important role in the rejection of the mother country.⁹⁹ The first colonists to break away from France were not the turbulent and ambitious “Americans” of Saint Domingue but the traditionalist planters of the Windward Isles, who rejected the French Republic in September 1792. While the colonial world had its own social and economic imperatives, historians of France’s revolution can find much that is familiar in the revolutionary experience of the French overseas. Much less is known about the free coloreds. Their rapid growth in population suggests that, revolution or not, sooner or later they would have become a potent political force. The French Revolution, however, gave them a public forum unavailable in the colonies and the Declaration of Rights seems to have had an immediate impact on their platform. When Ogé rebelled, moreover, he used as a rallying cry the decree of March 1790.

The relationship of the slave population to the French Revolution remains obscure because slaves had little opportunity to express their views in durable form. Césaire was wrong to connect the Martinique uprising of August 1789 to news of the Bastille’s fall; it was then still unknown. Nevertheless, other rumors, conflating abolitionism and royalist reforms, seem to have been important in mobilizing the rebels.¹⁰⁰ These circulated in Saint Domingue late in 1789, along with talk of “the revolt of the white slaves [in France] who had killed their masters and taken possession of the land.” Most important, these rumors helped to instigate the great insurrection of 1791.¹⁰¹ Yet they were not confined to the French islands alone. One of the intriguing features of the period from 1789 to 1791 is the emergence of a new type of slave revolt, soon to become common, in which the rebels claimed to have been already freed.¹⁰² The evidence concerning the aims of the Saint Domingue rebels of 1791 is contradictory. However, the commitment of the slaves’ leaders to the overthrow of slavery was ambiguous and rarely expressed in idealistic terms. Leaders tended to adopt a conservative church-and-king rhetoric and express contempt for those they called “les

⁹⁹ Geggus, *Slavery, War and Revolution*, chap. 3.

¹⁰⁰ Pierre Dessalles, *Historique des troubles survenus à la Martinique*, ed. Léo Elisabeth (Fort de France, n.d.), 17–30; Henri Lemery, *La Révolution Française à la Martinique* (Paris, 1936), 22–26.

¹⁰¹ Exactly how such rumors functioned, and their relative importance for leaders and masses, raises the intractable problem of the psychology of subordination, which is likely to remain insoluble; David Geggus, “The Causation of Slave Rebellions,” *Indian Historical Review*, 14 (1987–88), forthcoming.

¹⁰² Although a few earlier examples exist, the phenomenon fully emerged with the revolts or conspiracies in Martinique (1789), Tortola (1790), Dominica, Port Salut and the northern plain in Saint Domingue (1791). The last examples I am aware of come from Jamaica (1831) and Bourbon (1832).

citoyens," which complicates considerably the question of their relationship to the French Revolution. Although Eugene Genovese has argued for a new "bourgeois-democratic" type of slave revolt developing under the revolution's influence, I suggest that what is novel about slave resistance in this period most probably derived from the international antislavery movement and late *ancien régime* reformism.¹⁰³

THE INTERACTION BETWEEN EVENTS IN PARIS AND THE CARIBBEAN during the closing months of the Constituent Assembly ensured the colonial question a much greater salience within revolutionary politics in the months to come. Curiously, the first news of the August uprising that reached France was regarded with considerable skepticism, and the concurrent reports of the white secessionist response (this time, real) often seemed just as threatening.¹⁰⁴ Yet it was the slaves that rebelled in this last month of the Constituent Assembly who would bring not only racial equality and slave emancipation to the French empire but also colonial secession and, in 1804, its first independent state.

¹⁰³ Eugene Genovese, *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the Modern World* (Baton Rouge, La., 1979); Geggus, "French and Haitian Revolutions."

¹⁰⁴ *Moniteur universel* (November 1, 1791); Jacques-Pierre Brissot, *Discours de J.-P. Brissot . . . sur les causes des troubles de Saint-Domingue* (Paris, 1791), 4; Milscent, *Sur les troubles*, 3, 11–14.

Commencing the Third Century of Debate

JACK R. CENSER

LIVELY DEBATES ABOUT THE FRENCH REVOLUTION AND ITS IMPACT have filled scholarly journals and book reviews this bicentennial year of 1989. Striking even to non-specialists is the extent to which politics and ideas have displaced social and economic forces in most of these works. Also notable is the diversity of their authors. For many years, historians interested in the revolution almost always looked to the French for leadership and direction, but, during the last few decades, foreign scholars have begun to move into the limelight.¹ The following pages focus on six prominent representatives of this new generation, five from England or North America: François Furet, Lynn Hunt, John Bosher, William Doyle, Simon Schama, and D. M. G. Sutherland.

Before looking at the most recent scholarship, I will first discuss the classic works of Georges Lefebvre, for his successors continue to debate the tradition he created.² Lefebvre's overview most often is labeled Marxist because class conflict lies at its core. He attributed the revolution of 1789 to the separate actions of four distinct social groups: the nobility, the bourgeoisie, the urban working class, and the peasants. Although often considered the monarch's natural ally, the nobles, the first active opponents of long-term royal efforts to monopolize power, forced the calling of the Estates General. Encouraged by their example, the bourgeoisie escalated the conflict. According to Lefebvre, the middle class, bolstered by its strong economic position, wanted merit to replace the privileges of birth. As the battle against royal authority thus widened into an attack on privilege, it attracted still other social groups. Both the urban working classes and the peasantry had long despised the decadent, snobbish aristocracy.

In writing this article, I incurred a great many debts: to colleagues at the Max-Planck-Institut für Geschichte, to Keith Baker, Shaul Bakhash, Hans Erich Bödeker, and Lynn Hunt. My greatest appreciation goes to Jane Turner Censer, who helped me refine both my ideas and my prose. Of course, I take full responsibility for any remaining errors.

¹ For an excellent review of works on French history by Germans, see Jeremy Popkin, "Recent West German Work on the French Revolution," *Journal of Modern History*, 59 (1987): 737–50.

² George Lefebvre's studies include: *Les Paysans du Nord* (Paris, 1972); *Questions agraires au temps du Terreur* (Strasbourg, 1932); *The French Revolution*, 2 vols., vol. 1: *From Its Origins to 1793*, trans. Elizabeth Moss Evanson, vol. 2: *From 1793 to 1799*, trans. John Hall Stewart and James Friguglietti (New York, 1962–64); *The Thermidoreans and the Directory: Two Phases of the French Revolution*, trans. Robert Baldick (New York, 1964); *Napoleon: From Eighteen Brumaire to Tilsit, 1799–1807*, trans. Henry F. Stockhold (New York, 1969); *Napoleon: From Tilsit to Waterloo, 1807–1815*, trans. J. E. Anderson (New York, 1969); and *The Coming of the French Revolution*, trans. Robert R. Palmer (Princeton, N.J., 1947).

Consequently, when in July 1789 the king's actions threatened the National Assembly, the people of Paris revolted in defense of the bourgeoisie. Later, fearful of an aristocratic counterattack, the peasants destroyed the contracts and records that underlay the old manorial regime. Eventually, the bourgeoisie succeeded in eliminating Old Regime privileges, an effort exemplified by passage of the Declaration of the Rights of Man and Citizen.

According to Lefebvre, even before 1789 ended, the initial alliance that made the revolution had begun to disintegrate. It was replaced by a class struggle—mainly between the bourgeoisie and the urban workers. Angered by the self-interested bourgeoisie's domination of the National Assembly, the more egalitarian Parisian workers (especially the self-proclaimed *sans-culottes*), with help from an unlikely partner, the middle-class Jacobins, fought for control of the Assembly. The summer of 1793 found the *sans-culottes* allied with the Jacobins to advance a radical social and political program. Although Lefebvre credited the *sans-culottes* with the Jacobins' ascendancy, he was reluctant to assign them responsibility for the Reign of Terror that followed. Instead, he blamed circumstances beyond their control, especially the warfare waged within the country and outside its borders.

To explain the fall of the Jacobins and the emergence of the more moderate Directory, Lefebvre turned again to notions of class conflict. The relationship between the Jacobins and Parisian workers deteriorated when the Jacobins, at least somewhat true to their class origins, refused to accede to the economic demands of artisan workers. Eventually, on 9 Thermidor (July 27, 1794), the middle class, through its representatives in the national legislature (the Convention), grasped power and held it through the Directory (1795–1799). Maintaining authority was made difficult by continued opposition from the *sans-culottes* and from revitalized royalists. This last group was dominated by former members of the nobility but included wealthy commoners who now longed for the return of Old Regime. Also to the detriment of the prerevolutionary bourgeoisie, problems of war and poverty created enemies for their government among all social classes. When representatives of the middle class turned to the military for assistance, they inadvertently prepared the way for Napoleon, a soldier who acquired power by promising to implement the bourgeoisie's social agenda.

Lefebvre's detailed description of social struggles notwithstanding, his overall view of the revolution was highly positive. The Declaration of the Rights of Man and Citizen, he declared, was a great milestone in history. Despite its bourgeois authors, this document, created during a period of relative social harmony, decreed a society open to all. Even when the revolution later drifted into problems, Lefebvre did not lose faith in it. Although he disapproved of the bourgeoisie's narrow self-interest, he found its contribution to the development of capitalism important. The same mix of praise and apologia characterized his interpretation of the Terror.

Inevitably, Lefebvre's work produced controversy, which intensified during the mid-1950s. Although French scholars eventually joined the attack, Anglo-

American historians, including Richard Cobb, Alfred Cobban, Robert Forster, and George V. Taylor, led the way.³ Later called "revisionists," most of these researchers accepted Lefebvre's emphasis on social causation but questioned his characterizations of the nobility, bourgeoisie, urban workers, and peasantry. The more radical among this group, such as Cobb, also challenged Lefebvre's grounding of political behavior in class interests.

For the most part, the revisionists' work undermined Lefebvre's classic interpretation but failed to replace it with a new social explanation or an alternative political account. Cobb's work pointed toward the political, but over time he retreated from offering any explanation at all. Cobban's famous *Social Interpretation of the French Revolution* (1965) was no more than an extended essay. Despite M. J. Sydenham's precocious work *The French Revolution* (1966) and J. M. Roberts's *French Revolution* (1978), revisionism until recently connoted attack more than reconstruction.

Although most revisionists avoided making general statements about the revolution, their stories frequently suggested less positive interpretations than Lefebvre's. For example, Cobb portrayed the people of France as material beings interested in food, love, and fun. For them, the revolution's disruption of authority represented an unwelcome intrusion into their lives. From such a perspective, the revolution lacked luster as an ennobling event. Cobban also diminished the revolution by depicting it as a chance occurrence, more an epiphenomenon than the inevitable outcome of massive social forces.

Through the 1960s and 1970s, defenders of Lefebvre's perspective continued to compete with the revisionists over how best to interpret the revolution. Albert Soboul, Lefebvre's successor as doyen of the Marxist school, spent countless hours and pages refuting his critics. But he did not advance much beyond Lefebvre and, in many respects, reified and hardened some of Lefebvre's stances.⁴

IN THE LAST DECADE, HOWEVER, MARXIST HISTORIANS have moved in some new directions, most notably toward a concentration on cultural history. Of particular note have been Michel Vovelle's investigations of popular *mentalités*, which depend on imaginative uses of iconographic sources. Jean-Paul Bertaud, perhaps best known for his study of the army, has contributed to the history of the press, while Jacques Guilhaumou has borrowed from linguistics to explore the

³ For important examples of their work, see Richard Cobb, *The Police and the People: French Popular Protest, 1789–1820* (Oxford, 1970); Cobb, *Reactions to the French Revolution* (Oxford, 1972); Alfred Cobban, *The Social Interpretation of the French Revolution* (Cambridge, 1965); Cobban, *Aspects of the French Revolution* (New York, 1968); Robert Forster, *The Nobility of Toulouse in the Eighteenth Century* (Baltimore, Md., 1960); and George V. Taylor, "Noncapitalist Wealth and the Origins of the French Revolution," *AHR*, 72 (1967): 469–96. For a French revisionist, see Guy Chaussinand-Nogaret, *La Noblesse au XVIII^e siècle: De la Féodalité aux lumières* (Paris, 1976).

⁴ For example, see Albert Soboul, *A Short History of the French Revolution, 1789–1799*, trans. Geoffrey Symcox (Berkeley, Calif., 1977).

revolution's intellectual framework. Yet even this newest generation of Marxists work within Lefebvre's general framework.⁵

In this essay, I have chosen to focus on work that is neither Marxist nor part of the violently antirevolutionary writing that has appeared as part of the bicentennial, such as that of Pierre Chaunu. Chaunu and his followers view the suppression of counterrevolution in the Vendée as a form of genocide and stress the tremendous financial and human costs of the revolution. Often, they portray the French Revolution as the precursor of brutal twentieth-century leftist dictatorships, from that of V. I. Lenin to the regime of Pol Pot.⁶

Most highly touted of the new scholars have been François Furet and Lynn Hunt. Their work is part of a more general emphasis on politics and ideology that has helped to divert the increasingly unproductive debate between Marxists and revisionists into new channels. The limited space of this essay precludes discussion in detail of the research of such other notable exponents of this new trend as Keith Michael Baker and Patrice Higonnet.⁷ Although their studies resemble Furet's, each makes notable contributions on his own. Higonnet's interest in America as well as France highlights a comparison at most implicit in Furet's corpus.

Of all the major new overviews of the French Revolution, Furet's was the first to appear. Without question, he was the person most responsible for initiating a new paradigm of explanation. In the early 1970s, he began to develop an alternative to Lefebvre. A full version of his schema appeared in *Interpreting the French Revolution*, published in France in 1978. Subsequently, he attempted to rehabilitate a number of nineteenth-century historians who offered alternatives to Marx.⁸ During the bicentennial period, Furet once more has turned to work on the revolutionary decade, in the form of press interviews, colloquia, a new chronological overview, and two co-edited books, one a dictionary of essays on vital concepts and the other a collection of the speeches given in the Constituent

⁵ For important works, consult Michel Vovelle, *The Fall of the French Monarchy, 1787–1792*, trans. Susan Burke (New York, 1984); Vovelle, ed., *Les Droits de l'homme et la conquête des libertés* (Grenoble, 1988); Vovelle, ed., *Les Images de la Révolution française* (Paris, 1988); Vovelle, *La Mentalité révolutionnaire: Société et mentalités sous la Révolution française* (Paris, 1985); Jean-Paul Bertaud, *Les Amis du Roi: Journaux et journalistes royalistes en France de 1789 à 1792* (Paris, 1984); Bertaud, *La Révolution armée: Les Soldats-citoyens et la Révolution française* (Paris, 1979); Jacques Guilhaumou, *La Langue politique et la Révolution française: De L'Evenement à la raison linguistique* (Paris, 1988).

⁶ Pierre Chaunu has given numerous interviews to the press on the revolution. For the tenor of his remarks, see *World & I*, 2 (1987): 662–71. For influential contributions to this point of view, see also Reynauld Secher, *La Genocide franco-français, la Vendée-Vengé* (Paris, 1986); and René Sedillot, *La Coût de la Révolution française* (Paris, 1987).

⁷ Keith Michael Baker, *Inventing the French Revolution: Essays on French Political Culture in the Eighteenth Century* (Cambridge, forthcoming); Patrice Higonnet, *Sister Republics: The Origins of French and American Republicanism* (Cambridge, Mass., 1988).

⁸ *Interpreting the French Revolution* first appeared as *Penser la Révolution française* (Paris, 1978). Cambridge University Press published the English translation by Elborg Forster in 1981. This book and the articles that led up to it differ markedly from Furet's first overview of the revolution, written with Denis Richet, which might be categorized as revisionist; *The French Revolution*, trans. Stephen Hardman (New York, 1970). For Furet's research on nineteenth-century historians, see *Marx et la Révolution française* (Paris, 1986); and *La Gauche et la Révolution française au milieu du XIX^e siècle: Edgar Quinet et la question du Jacobinisme* (Paris, 1986).

Assembly.⁹ This most recent spate of work at least partially answers those critics who claimed that his brilliant hypotheses were unrelated to empirical efforts.

For Furet, ideas have the power that Lefebvre attributed to social class. Thus, in *Interpreting the French Revolution*, he presented a story of competing ideologies. While particular interests may have been served by specific discourses, agendas in the political arena were set by belief systems or “political languages.” Modes of speech—not interests—played the determining role. Furet began by describing (and defending) the prerevolutionary ideological status quo as a sort of traditional liberalism. Although revolutionaries criticized this philosophy for its production of a system of special privileges, Furet argued that they did not understand the benefits of such privileges. Under the Old Regime, he claimed, residency in a city or participation in a guild, just like the rank of nobility, served to protect individuals’ freedom (here defined as a lack of oppression).

Problems began when absolutist monarchs, eager to increase their authority, attempted to do away with such traditional protections. Although many privileges survived, the network of legal support—the parlements, municipalities, guilds, and manors—became isolated or limited. With traditional privileges under attack, French society became particularly vulnerable to the appeals of new political ideologies.

In this situation of flux, Rousseauian notions of equality, best explained in *The Social Contract*, emerged as the strongest competitor to traditional liberalism and its system of privileges. For Furet, this was not a promising development. Following Thomas Hobbes, Furet argued that absolute equality leads either to chaos or dictatorship. During the closing decades of the Old Regime, Rousseauian ideas won the battle for the political heart of France, thanks largely to the actions of the king; for, as a result of the king’s efforts at centralization, never had the French more resembled each other than in the years before 1789.¹⁰ The increasing reality of equality encouraged wide acceptance of Rousseauism, even though its converts had little political experience and remained mostly philosophical, not practical, in their thinking. Following the lead of Augustin Cochin, Furet then described the flowering of a new social world, reflective and supportive of this ideology.¹¹

Although this battle of ideas long pre-dated 1789, it took the financial crisis and the resultant elections to the Estates General to settle the conflict. Those political contests were so organized by the monarchy as to mix traditional conceptions with the new Rousseauian notions of equality. On the one hand,

⁹ For sample articles on Furet, see the *Los Angeles Times*, February 2, 1989, and *Süddeutsche Zeitung* (Munich), June 10–11, 1989. The colloquia he organized met in 1986, 1987, and 1988 in Chicago, Oxford, and Paris respectively, and volumes of the proceedings are being published under the general title, *The French Revolution and the Creation of Modern Political Culture*, by Pergamon Press. Volume 1 (1987) and volume 2 (1988) have already appeared. The chronological overview is *La Révolution de Turgot à Jules Ferry, 1770–1880* (Paris, 1988); the two co-edited books are, with Mona Ozouf, *Dictionnaire critique de la Révolution française* (Paris, 1988); and with Ran Halévi, *Orateurs de la Révolution française*, vol. 1. *Les Constituants* (Paris, 1989).

¹⁰ For this position on the monarch’s role and its effect, Furet depended on Alexis de Tocqueville, *The Old Regime and the French Revolution*, trans. Stuart Gilbert (1856; Garden City, N.Y., 1955), 77–81.

¹¹ Augustin Cochin, *Les Sociétés de pensée et la démocratie: Etudes d’histoire révolutionnaire* (Paris, 1921); Furet, *Interpreting the French Revolution*, 164–204.

various customary privileges, such as separate meetings of the estates, were allowed to continue. On the other, by linking the number of representatives to population figures and encouraging campaigns for votes, the monarchy recognized the principle of equal roles. As elections continued, the political process became increasingly egalitarian, with Rousseauists in charge.

Eventually, Furet argued, authority was for the first time formally entrusted to the people, with extraordinary results. In effect, royal power had ceased with the financial crisis in 1787, but it took two years for the people fully to appreciate what had happened. Then, in the midst of the revolution, with any actual exercise of authority believed to be the culprit for the ills of society, "language was substituted for power, for it was the sole guarantee that power would belong only to the people, that is, to nobody."¹² With politics reduced to a linguistic struggle and Rousseauian thinking dominant, politicians could no longer exercise power in the traditional sense. Although some resisted, in the end they were forced to compete in the arena of discourse. The most influential revolutionary leaders clearly understood that if they were to play the game of politics, they had no choice about its rules. Leadership passed to those who promised the greatest subservience to the people and the greatest equality. Once extreme notions of equality that denied individual differences dominated political discourse, the coming of the Terror seemed inevitable. Fortunately, Furet went on, the Thermidoreans broke the tyranny of discourse and returned politics to competition among classes and political groups. Implicit in such a comparison of pre-Thermidor and post-Thermidor was Furet's negative evaluation of the revolution.

In the years after the publication of his suggestive synthesis, Furet's fertile and active imagination led him toward revisions, most often incremental rather than general in nature. For example, in the introduction to his book on the orators of the Constituent Assembly, co-edited by Ran Halévi, Furet modified his initial critique of the legislators' inexperience so as to give more credit to their accomplishments.¹³ Thus he strengthened his argument without changing its essential emphasis on ideological struggle. And he has been equally successful in pursuing a new view of the monarchy. His early portrayal of a monarchy strong enough to undermine a society of privilege failed to account for its inability by 1787 to keep the monarchy alive except in name. Furet later explained that the attack on privilege eventually wounded the monarchy, an institution that, despite its own efforts to level privileges, also depended on this system of rights.¹⁴

In other areas, Furet made even larger adjustments without abandoning his general thrust. For example, he first declared that the elections of 1788–1789 essentially settled the contest between traditional liberalism and Rousseauism and disclaimed interest even in Abbé Sieyès's views during the National Assembly. More recently, Furet extended the critical period of debate to include

¹² Furet, *Interpreting the French Revolution*, 48.

¹³ Furet and Halévi, *Orateurs*, xvi–xviii.

¹⁴ Furet, *La Révolution*, 46–59.

the late summer and autumn of 1789.¹⁵ In their work on orators, Furet and Halévi argued that the transformation of the Estates General into the National Assembly on June 17, the abolition of privilege on August 4, and the votes in late August and early September (on the Rights of Man, the royal veto, and bicameralism) constituted the “three great steps.”¹⁶ At each of these points, the two sides confronted each other, and, Furet and Halévi concluded, the Rousseauists triumphed each time. In the end, an egalitarian world of isolated citizens replaced the society of orders.

Although Furet offered a new and provocative overview of the conflict of political ideologies in 1789, Keith Baker furnished the details of the battle. In an article that accepted Furet’s general scenario, Baker traced traditional liberals’ efforts to contest the Rousseauists on a number of issues concerning the relationship between the constitution and the people.¹⁷ Leading the defense against equality was Jean-Joseph Mounier, who, though a moderate, became in the course of the debates interested in protecting parts of the traditional order. According to Mounier, France already had elements of a constitution. The Assembly’s task was to build on them in such a way as to preserve parts of the past state, including such inequalities as the primacy of the monarch. But his opponents, led by Jérôme Pétion and Sieyès, insisted that the equal citizens, making up the sovereign people, existed prior to any constitution, which, therefore, could be made and unmade at will. Baker insisted that the ultimate victory of the Rousseauists here and in more tangential matters had devastatingly negative consequences for the power and stability of the written constitution in France. Not surprisingly, such a flimsy document could little resist the ruthlessness and illegalities of the Year II. In thus tracing the increasing power of Rousseauian egalitarianism, with its corollary of popular sovereignty, through the Assembly debates into the autumn of 1789, Baker not only filled out Furet’s picture of the competition among ideologies but also tightened the link between Rousseauian notions and the Terror. Furet’s work of the 1970s showed how revolutionary politicians were compelled to promise an ever more radical egalitarianism, while Baker’s article spelled out more ways in which Rousseauian ideas could encourage abuses by undermining legal restraints.

Furet’s newer publications also expand his presentation of the people. Initially, he tended to portray the populace as faceless supporters of a revolutionary dynamic moving inexorably toward radical equality. In more recent studies, the people turn up at specific times and places, most often to lend a hand to Rousseauist advocates. But, like all other interest groups, the working classes

¹⁵ As late as 1987, he was still arguing that, in the elections, the Rousseauists had already triumphed. See François Furet, “La Monarchie et le règlement électoral de 1789,” in *The French Revolution and the Creation of Modern Political Culture*, vol. 1, Keith Michael Baker, ed., *The Political Culture of the Old Regime* (Oxford, 1987), 375–86.

¹⁶ Furet and Halévi, *Orateurs*, lix. On this shift, see also Ran Halévi, “La Révolution constituante: Les Ambiguïtés politiques,” in *The French Revolution and the Creation of Modern Political Culture*, Colin Lucas, ed., vol. 2, *The Political Culture of the French Revolution* (Oxford, 1988), 69–85. This article is reprinted in English in Jack R. Censer, ed., *The French Revolution and Intellectual History* (Chicago, 1989), 139–51.

¹⁷ Keith Baker, “Fixing the French Constitution,” in his *Inventing the French Revolution*.

act only through their choice of a discourse. Language still dominates Furet's stage. The "key moment in 1789" happened not at the Bastille but at the Hall des Menus-Plaisirs, meeting place of the National Assembly.¹⁸

Most recently, in newspaper interviews, François Furet calls the revolution a great event that went astray. His intent here may be to distance himself from conservative critics who suggest that the revolution led not only to the Terror but also to the unspeakably greater immolations of the twentieth century.¹⁹ Yet, for the most part, in scholarly talks and essays, Furet does not alter his general theories about the origins of the revolution or change his rather negative assessment of its outcome.

LIKE FURET, LYNN HUNT IS INTERESTED IN LANGUAGE, but she tends to focus on subtle shifts in word usage and grammar rather than on the clash of ideas. For Hunt, language and image serve as barometers of revolutionary sentiment. In her pathbreaking work *Politics, Culture, and Class in the French Revolution* (1984), Hunt scrutinized the everyday language used to express the ideology of "democratic republicanism," which she found at the core of the revolutionary spirit.²⁰ Her concern was first to uncover and then to explain its underlying structure or grammar. To do so, she turned to a range of theoretical formulations, particularly to hypotheses regarding theatricality. She also borrowed insights from the works of literary critics, anthropologists, and linguists. She used their theories to help her arrange and rearrange the content of revolutionaries' language, to uncover their basic values, the deeply embedded categories of revolutionary thinking. Revolutionaries' intentions, she argued, could be inferred from their symbolic efforts. Hunt maintained it was important to investigate as well the social backgrounds of her revolutionaries or republicans (terms used interchangeably) and did so in the second half of her book. Her intent was not to develop causal or hierarchical models of the relations among class, language, and belief but to locate, if possible, "a fit or affinity" between them.²¹

Hunt's interest in discourse analysis strongly shaped her overview of the French Revolution. Within revolutionary thought, she argued, there was no past, only a "mythic present," composed of belief in the nation and the revolution.²² Propelled forward by fears of conspiracy, revolutionaries spoke and wrote first as if actors in a comedy, cheering their successes; then as if in a romance, struggling with good and evil; and finally as if in a tragedy, watching things fall apart. In all three stages, their thought was envisioned as transparent or hostile to artificiality. It was also ever vigilant and always subservient to the

¹⁸ Furet and Halévi, *Orateurs*, xxvi.

¹⁹ See note 9 above.

²⁰ Lynn Hunt, *Politics, Culture, and Class in the French Revolution* (Berkeley, Calif., 1984), 15.

²¹ Hunt, *Politics, Culture, and Class*, 13.

²² Hunt, *Politics, Culture, and Class*, 26–28.

people. Through the revolutionaries' use of symbols, they developed this litany of new beliefs.

But what political group was associated with such principles? Essentially, Hunt argued, it consisted of outsiders: individuals of education, wealth, and even connections, who, for reasons of religion, age, mobility, or external cultural and political ties, had not been thoroughly integrated into Old Regime society. For Hunt, this new political class gave "the symbolic forms of revolution . . . their motive force. Their newness to political affairs, their relative youth, and their position as relative outsiders had the effect of accelerating the development of the rhetoric and symbols of revolution." Yet—she warns those prone to statements of temporal causality—"the new men and the new political culture came into being together. In this case, it is fruitless to try to determine which came first."²³

In terms of their focus on discourse and some of their specific findings, Furet and Hunt resemble one another. But Hunt's discussion of the revolutionary notion of a "mythic present" and her examination of the stylized emotional responses of the revolutionaries when events turned against them provided insights into mental fluctuations—psychological responses to revolutionary challenges—quite different from the critique that emerged from Furet's analysis of the language of equality. Although engaged like Furet in studying revolutionary language for patterns in political rhetoric, she also examined much deeper matters. And, unlike Furet, she tied these transformations of political views and subterranean mental structures to certain social groups, not to any eighteenth-century heritage. But, in her moral evaluation, she tended to move closer to Furet. For the most part, in *Politics, Culture, and Class*, Hunt played the neutral observer, impressed by the inventiveness of the revolution. On occasion, however, she pictured the revolution as a profoundly mixed experience associated with both political participation and authoritarianism.

In Hunt's more recent research,²⁴ she looks again at republican rhetoric, this time using feminist categories of analysis, in order to uncover a revolutionary grammar of gender relations. Once more, she invokes theory to help explicate texts. Where she had earlier relied, somewhat cautiously, on literary theory, she now boldly seizes upon Freudian conceptualizations, especially as formulated by René Girard, and begins to move toward an explication of the revolutionary unconscious. She also has expanded her documentary base so as better to plumb the depths of the soul.²⁵ Pervading revolutionary images, Hunt argues, were "unconscious fantasies about the familial order underlying revolutionary politics."²⁶ Particularly significant is the Freudian concept of the family romance

²³ Hunt, *Politics, Culture, and Class*, 214, 216.

²⁴ Three lectures from the Gauss Seminars in Criticism, Princeton University, November–December 1988, as yet unpublished, provide an important statement of her viewpoint. Their titles are: "The Revolution without Lineage"; "The Many Bodies of Marie-Antoinette"; and "Politics in the Boudoir: Sexuality and Republicanism in the Marquis de Sade."

²⁵ Lynn Hunt, "The Political Psychology of Revolutionary Caricatures," *French Caricature and the French Revolution, 1789–1799* (Los Angeles, 1988), 33–40.

²⁶ Hunt, "Political Psychology," 36.

of power. The king and queen are driven out and replaced by the band of brothers.

In the caricatures of the republican period . . . the revolutionaries tended to portray themselves as young people without parents and without children. They imagined themselves as part of no lineage. In a sense, this was a family romance with the notion of parentage taken out; only brothers and sisters remained. The revolutionary slogan of liberty, equality, and *fraternity* had a very distinctive meaning. In place of the one father-king, there were now many, equal brothers.

But fraternity certainly did not mean that brothers and sisters were equal partners . . . The elimination of the royal patriarchal couple did not automatically open the way for women to participate in public affairs; the proliferation of the female allegory was made possible, in fact, by the exclusion of women from public affairs. Women could be representative of abstract qualities and collective dreams because women were not about to vote or govern.²⁷

Wishing to be free of their political parents, the revolutionaries imagined a different kind of family, dominated by the children—especially the sons. But a family romance based on sons alone proved difficult to sustain.²⁸

According to Hunt, this new investigation of the gendered nature of revolutionary discourse complements the observations offered in *Politics, Culture, and Class*. Whether she can integrate new notions about gender and politics with her earlier descriptions of outsiders remains to be seen. Of greater consequence is her shift from a primary focus on revolutionary political discourse to a primary focus on the revolution as a profound change in deep—even subconscious—mental structures, still likely to be linked to certain societal supports. In so doing, she moves far away from the sort of issues that most interest Furet toward a consideration of *mentalités*, even of fundamental psychological states. While continuing to share Furet's negative assessment of the revolution, her specific misgivings are very different from his. It is hard to appreciate the heroism of deeply misogynist revolutionaries whose releasing of the genie of revolution was, at best, an uncertain blessing. Thus, despite their somewhat different emphases, Furet and Hunt agree on the problematic nature of the French Revolution.

ALSO CRITICAL OF THE REVOLUTION but for quite different reasons are the authors of four recent narrative histories: William Doyle, D. M. G. Sutherland, Simon Schama, and John Bosher.²⁹ Although they do not constitute a formal school, these historians share a common background: a rejection of Marxist categories, an indifference to theory, a negative assessment of the revolution, and some consensus about how best to understand it. They also have direct links to the revisionist critics of Lefebvre. Both Doyle and Sutherland earlier pro-

²⁷ Hunt, "Political Psychology," 39.

²⁸ Hunt, "Political Psychology," 39.

²⁹ J. F. Bosher, *The French Revolution* (New York, 1988); William Doyle, *The Oxford History of the French Revolution* (Oxford, 1989); Simon Schama, *Citizens: A Chronicle of the French Revolution* (New York, 1989); and D. M. G. Sutherland, *France, 1789–1815: Revolution and Counterrevolution* (New York, 1986).

duced revisionist monographs.³⁰ Even though Boshier's and Schama's previous work did not conform wholly to the revisionist mold, they have strong personal links to revisionism. Boshier was a student of Cobban; Schama, a disciple of Cobb, warmly thanks Cobban for inspiration.³¹

Understanding the affinities among these overviews first requires separate analyses of each work. In his *Oxford History of the French Revolution* (1989), William Doyle begins by describing the prerevolutionary formation of an elite composed of a unified bourgeoisie and the nobility. Distressed by the Maupeou coup of 1770–1771 and convinced of the monarchy's despotism, the elite was encouraged by the financial collapse of the late 1780s to extract political concessions from the monarchy. At this point, the revolutionaries-to-be looked like idealistic reformers. Yet the tradition did not last. Although it had proved a successful political combination, the process of elections to the Estates General divided the elite along class lines. Furthermore, the elections and the passions surrounding them had aroused the formerly acquiescent working classes—both rural and urban—and they pressed the bourgeoisie not only to insist on its demands but to go further and address some needs of the working classes.

In contrast to Lefebvre, who attributed the fall of the Old Regime to the accumulation of long-term grievances (the nobility against the crown and all against the aristocracy), Doyle argues that those wishing change before 1788–1789 constituted a social amalgam of nobles and middle class and that the working classes became hostile to the status quo only during the revolutionary crisis. Thus, to explain the demise of the Old Regime, Doyle replaces Lefebvre's view of four fully formed prerevolutionary classes, each having a set social agenda, with a reforming elite that lacked social cleavages and a quiescent poor. In his narrative, only late in 1788 do the classes begin to behave much as in the classic interpretation.

Although accepting Lefebvre's view of the revolutionary crisis of 1788, Doyle's construction of the subsequent years represents another departure. He shows various legislatures replacing the bourgeoisie as the centers of activity. And, in his account, the National Assembly (1789–1791) created difficulties for its successor (the Legislative Assembly, 1791–1792) by ill-advised actions that had unanticipated effects—particularly the settlement with the church. Such problems generated hostilities that cut across classes. The existence of a full array of enemies, inside and outside the country, stimulated the bellicosity of the

³⁰ Sutherland, in his book *The Chouans: The Social Origins of Popular Counterrevolution in Upper Brittany* (Oxford, 1982), and in his numerous articles on the counterrevolution, disputes the Marxist belief that social strains between haves and have-nots led to counterrevolution. Instead, he focuses on the failure of the revolutionaries to provide relief for both rich and poor renters and on the way traditional communities resented the revolutionary intrusion into religious matters. Doyle, in *The Parlement of Bordeaux and the End of the Old Regime 1771–1790* (London, 1974), and his articles, "Was There an Aristocratic Revolution in Pre-revolutionary France?" *Past and Present*, 57 (1972): 97–122; and "The Parlements of France and the Breakdown of the Old Regime, 1771–1788," *French Historical Studies*, 6 (1969–70): 415–58, concentrates on the parlements. Instead of the tendency among Marxists to see the parlement as representing only the nobility in order to advance the narrow claims of that class, this author sees the French as viewing the parlement as a genuine bulwark of freedom against monarchical claims.

³¹ Schama, *Citizens*, 906.

legislature. Then the war, and its failures, led to the political rift in the Assembly between the radical Jacobins and more moderate Girondins and to the rise of the sans-culottes as a political force. Together with the Jacobins, these Parisians installed a radical government. In this scenario, politics provides the motive force, with only the appearance of the sans-culottes resembling the classic interpretation. Doyle goes on to provide new explanations of the Terror, the waning of radicalism, and the collapse of the Jacobin regime in 1794. He emphasizes general exhaustion as the cause of this retreat in contrast to Lefebvre's stress on the social conservatism of the bourgeoisie. Doyle concludes that the unpopularity of the Directory, the government established after the fall of Robespierre, led to the rise of Napoleon. Dominated by the executive, the Directory lacked a constitutional method to solve the many problems ailing France. Napoleon proved successful because he promised something to all those dissatisfied with the government, not just to a particular class.

Largely dismissing class as a causal factor, Doyle usually substitutes ideology for the period before 1789 and politics for the years after. In contrast to Lefebvre's systematic preference for social factors, Doyle is more eclectic, empirical, and balanced. He also is markedly less theoretical and dogmatic. In the end, Doyle judges the revolution more harshly than do Marxists such as Lefebvre. While some good issued from the revolution, for most of the population, things were worse than before.

In many respects, D. M. G. Sutherland's view of the French Revolution (1986) is echoed in Doyle's. Although Sutherland tends to stress the counterrevolution more than the war as the cause of the struggle within the political elite and in Paris, he and Doyle differ mainly in emphasis. The one major exception is Sutherland's view of Napoleon. Whereas Doyle views Napoleon as the efficient administrator, Sutherland depicts the dictator's rise as the result of governing officials' efforts to retain power. For Sutherland, the revolution thus becomes a means of extending the radical centralization schema introduced by the monarch. Those seeking an elegant political narrative should consult Doyle; Sutherland is particularly rich in analysis. And each emphasizes different subjects. Sutherland draws on his own research in the provinces to show the revolution outside Paris. Although Doyle knows the provinces well, his specialty is the war.

While Doyle's and Sutherland's narratives closely resemble each other, those offered by Simon Schama (1989) and John Bosher (1988) share some features. But not in style: Schama's massive book offers numerous close-ups of individuals, excursions into culture, images, and court gossip, and a remarkable command of the language. Wit, word choice, and verve explain some of this work's appeal. This elegant work experiments linguistically and organizationally, in contrast to the more straightforward narratives by Bosher as well as by Sutherland and Doyle. Schama devotes almost half of his book to the Old Regime, pictured as a dynamic society led by a reformist government. He seems to find it somewhat inexplicable that anyone objected to the eighteenth-century French government and avers that no one had deep-seated grievances. Unexpectedly, from the social elite—both noble and common—complaints welled up.

Those who railed against the Old Regime owed much of their thirst for change to the organizing, improving, rationalistic, and even liberalizing absolutist monarchy. While they contemplated change, they never envisioned the whole-sale abolition of privilege. Still, weakened by the financial collapse, the monarchy fell. If only the monarch had been more tactful, muses Schama, things might have worked out differently.

For Schama, late 1788–1789 was a period of sharp transitions. While elite reformers remained committed to a restrained regime consistent with continued privilege or at least some kinds of inequality, a coherent competing group emerged in support of legal equality. Also members of the elite, these new entrants spoke an entirely different language of passionate hostility to inequality. The emotion and substance of their appeal played well to a populace that, according to Schama, instinctively and impulsively gravitated toward egalitarian—and thus anti-liberal—economics and politics. Once drawn in, the lower classes gave way to their inherent anger and bloodlust. Such a complex mix of passion with popular participation and values, Schama argues, gave the revolution even in 1789 both a radical and a bloody character. “The Terror,” he recounts, “was merely 1789 with a higher body count.”³²

From its inception, then, Schama’s revolution, stemming from an alliance between the populace and the new breed of politicians, was steeped in violence. The period after 1789 simply extended an earlier dynamic, with the vast majority of the elite continuing to engage in passionate, egalitarian rhetoric and the populace, increasingly the Parisian sans-culottes, responding with ferocity. The willingness of politicians to compete and outdo one another in crowd appeals apparently drove the revolution to new heights. The radical Jacobins led by Robespierre were but the last and most histrionic. Thus the revolution was propelled forward by an unholy fusion of ever more radical language with ever broader popular participation, until the Thermidoreans broke the spell.

Schama’s explanation of the revolution makes clear his estimate of it, for he links the revolution to terror, anti-intellectualism, and antiliberal beliefs. If readers continue to doubt Schama’s intentions toward the revolution, they have only to consult two contrasting vignettes. Schama pictures Marie-Antoinette, after she was sentenced to death, as a gallant figure and doting mother. But he displays scant tolerance toward another prominent woman of the period, the revolutionary Théroigne de Méricourt, who had a nervous breakdown in 1793 and never recovered. In the last paragraph of his book, he proclaims, “Sympathy seems out of place here for in some sense the madness of Théroigne de Méricourt was a logical destination for the compulsions of revolutionary idealism.”³³

Some of Schama’s themes reverberate in the pages of Boshier’s *French Revolution*, but the two follow different tracks often enough that Boshier merits separate consideration. Enamored of Old Regime society and government, Boshier—like Schama—wonders about the source of the discontent of the noble

³² Schama, *Citizens*, 447.

³³ Schama, *Citizens*, 875.

and middle-class elite. Boshier argues that royal mistakes allowed such complaints, rather moderate on the whole, to erupt into an insurrection that a small measure of force could, and should, have stopped. Nonetheless, the revolutionaries gained power and, despite their unfortunate participation in an unworthy uprising, created a valuable enterprise. Although a bit vague on how Old Regime reformers instituted revolutionary change, Boshier appears to suggest that those in the opposition before 1789 gradually developed more advanced ideas. From this new position, they ushered in what he calls “the liberal interlude,” ending privilege by making all equal before the law, a move Boshier clearly favors.³⁴

To explain subsequent bloodletting, Boshier focuses on the radical Jacobins—Robespierre, Danton, Marat, Hébert, and others from the social elite—who, after 1789, encouraged for their own ends popular political aspirations. In so doing, they opened the doors to the *sans-culottes*, whom Boshier characterizes as a violent group, imbued with antiliberal political and economic habits. In 1793, the *sans-culottes*’ and the Jacobins’ lust for power overwhelmed the liberal legislators, who had managed until then to maintain control. In Boshier’s closing chapters, he edges toward an overall evaluation of the revolution in which he favors the early liberalism but dissociates himself from its radicalization.

Although they differ on many specifics, Doyle, Sutherland, Schama, and Boshier may be grouped together for good reason. First, like their intellectual forebears, all four reject Marxist accounts of the social origins of the revolution, Schama and Boshier being the most extreme in their avoidance of issues of class and their antipathy toward the *sans-culottes*. Second, all four reject Lefebvre’s optimistic assessment of the revolution. While early revisionists offered a highly qualified assessment of the revolution’s achievements, these new accounts are much more aggressively negative. Here, Doyle is the most circumspect. Boshier, while applauding the early revolution and Directory, becomes angry about the excesses at the height of the revolution. Sutherland plainly dislikes the drift of events; Schama intemperately condemns the entire affair, veering toward the extreme views of Pierre Chaunu and company. Cobban and Cobb never went so far.

Third, these authors chose similar strategies to describe and explain the revolution. Politics take center stage and extensive political chronologies fill their works. Yet they also consider social, political, intellectual, and economic factors, a common-sense approach typical of historians for whom theory is relatively unimportant. In particular, they make no attempt to deal with current debates about the relation of discourse to reality. They simply accept that these factors coexist, one alongside the other. Indeed, even when these authors disagree, they suggest only two major lines of interpretation. Doyle and Sutherland agree on most matters all the way to Napoleon. And, despite serious differences, Schama and Boshier offer similar interpretations of a number of issues, including the vitality of prerevolutionary France, its political flaws, and its rather unexpected

³⁴ Boshier, *French Revolution*, 133–57.

revolution. The disagreements become even less significant because of the originality of their shared viewpoint. They have virtually no precedents in this century, although one can find resonance between them and nineteenth-century critics of the revolution. Of course, they do disagree about the cause and the nature of the events of 1789. Boshier and Schama concur that prerevolutionary France was full of vitality and that the elite's criticisms were carping. For Boshier, a traditional elite simply grafted legal equality onto earlier goals and maintained a reformist tradition. Schama finds the revolution fundamentally different from the Old Regime and quickly linked to violence. He attributes such developments to an alliance between the populace and a new breed of politicians. And, in dealing with the next two-and-a-half years of the revolution, Schama and Boshier extend and augment their views of 1789. Boshier notes the rise of radical politicians and the sans-culottes but focuses on the efforts of the men of the assemblies to ward off attacks. Schama marks the turnover among revolutionaries, ending with the Jacobins as merely the worst of the lot. On events from mid-1792 until the Reign of Terror, whose importance as an era of violence remains unquestioned even by those (like Schama) who emphasize early extremism, the two agree again. They target the Jacobins, defined without reference to class, as the culprits in 1793. Boshier and Schama prove uncannily similar in their appraisal—and in their deep detestation—of the sans-culottes. Both, in their analyses of the legal terror of the Year II, assert that the bloodletting flowed from the same unholy alliance of politicians and common people responsible for earlier turmoil.³⁵

THE RECENT PROLIFERATION OF NEW INTERPRETATIONS, from Furet to Schama, is both exciting and bewildering. For almost every aspect of the revolution, several competing approaches vie for attention. How can one select among them? They seem to share only a rejection of Marxism and a skeptical attitude about the accomplishments of the revolution. And why are they so negative? While full answers to these queries require placing recent studies and their authors in political, personal, intellectual, and academic contexts, as well as the passage of time, preliminary comments are still possible.

The hostility to Marxism is not particularly difficult to explain. So much evidence undermines Lefebvre's interpretation that the authors of general syntheses pursue classic Marxist explanations only at great risk. Explaining the common pessimism about the revolution is more difficult. To be sure, many of the events of the years from 1789 to 1799 have since been condemned. But the vast scope of the revolution, its moral uncertainties, and the complexity of the events and of interpretations to explain them suggest that this consensus also

³⁵ While Schama and Furet agree as to the significance of language in the revolution, they strongly disagree as to its content and source. First, Schama believes that by 1788 the language of egalitarianism was triumphant, while Furet depicts a struggle lasting well into 1789 between Rousseauists and traditional liberals. Second, Schama treats the discourse of egalitarianism not as simply a language the populace had to use but as an accurate expression of their interests. In addition, Schama sees the Old Regime in far more positive terms than Furet does.

springs from shared subjective judgments. Some critics charge political bias, an expression of the political conservatism of the 1980s, but that is too crude an explanation. Historians generally do not offer roseate or bleak evaluations according to their mood. But conservatism, like any value system, may lead to particular queries or to the reconceptualization of evidence in a new and, some would aver, brighter light. Both Bosher and Furet specifically note that dismay over modern popular revolutions led them in part to a new perspective.³⁶ And Schama's sometimes overheated prose deprecating revolutionaries and cheering counterrevolutionaries suggests that he too may hold conservative values. Political inclinations—this time of the Left—also may be at work here. If one asks not why conservatives dislike the revolution but why progressives do not rise to support it, another insight emerges. During the last decade or two, many on the left have been more concerned with individual liberties than with social justice for all. Such an emphasis decreases the likelihood that scholars with leftist views will end up defending a revolution long on equality and short on liberty.

But not only political inclinations, seeping into historical judgments, bear responsibility. Traditionally, defenses of the revolution have stressed its long-term political and economic effects. Several of the revisionists, by inclination empirical, were more interested in the immediate, short-run, and often negative impact of the revolution. The emphasis on language in Furet, Hunt, and Schama—a bit more political because it tends to reduce interest in social and economic questions—encouraged negative judgments, because authoritarianism appeared earlier in rhetoric than in deed. Sutherland's background as a scholar of the counterrevolution encouraged a sensitivity to its proponents. Finally, Hunt's measure of the revolution along gender lines revealed another of its weaknesses. Thus variations in approach as well as in ideology have helped to generate a chorus of negative views of the revolution.

With so many cross winds combining in these new studies of the revolution, despite some similarities of evaluation, readers may easily end up more perplexed than enlightened. Such is the current state of play in a field long dominated by Georges Lefebvre's shadow. On the one hand, with such rich and detailed new alternative visions, it seems undesirable to favor one to the exclusion of others. On the other hand, it is likely that critics soon will begin just that task. Perhaps such an effort will reduce disorder while retaining the most profitable notions from the new scholarship.

In anticipation of such a thorough review, I would here like to mention those propositions that may prove the most enduring and at the same time indicate the kind of synthesis implied. Without a doubt, Doyle and Sutherland have produced the most comprehensive and least controversial of the accounts. Retaining those parts of the Marxist account that have survived challenge, they best replace the classic view. Additionally, their focus on the revolution beyond Paris—especially the war and the counterrevolution—sets an impressive standard. Such concerns help to make sense of the seemingly ineluctable transformation of the

³⁶ Bosher, *French Revolution*, x–xi; Furet and Ozouf, *Dictionnaire*, 13–16.

political scene. And recent works on the *cahiers* and on the National Assembly, all of which suggests slow but steady change, further document the gradual slide toward radicalism.³⁷ Still, Doyle and Sutherland accept too uncritically Doyle's hypothesis about the apparent despotism of the Old Regime. In contrast, Bosher and Schama envision this era as a period of great vitality, so effervescent that things got out of hand, although such excess was not necessarily inevitable. On the early revolutionaries themselves, Bosher has little to offer, as he is scarcely distinguishable from Doyle and Sutherland, and Schama's view ignores important distinctions among the politicians. Bosher's and Schama's treatments of the populace, flawed as they are by an almost obsessive dislike of the lower classes, are best left aside.

Finally, it seems to me that Furet and Hunt make important contributions to the reconceptualization of politics. Doyle's and Sutherland's treatments of the political center, while not incorrect, lack force. With the disappearance of class as an explanatory variable, the devastating splits among the revolutionary politicians are difficult to explain. The same may be said for the direction of policy after the great events of 1789. Both Furet and Hunt provide rationales: he for divisions among revolutionaries, she for their overall use of power. Perhaps these twin explanations—one mainly intellectual, the other more related to the revolutionary impulse—can be linked, although historians may be most comfortable with Hunt's earlier, and less theoretical, understanding of democratic republicanism. Her view of gender is dependent on modern theories that may not apply well to eighteenth-century revolutionaries. But, at the least, integrating the insights of Hunt and Furet into accounts such as those of Doyle and Sutherland provides the latter with a rationale for political action and divisions and the former with events that are needed to account for the development of ideas. This projected synthesis intentionally ignores Furet's characterization of the Old Regime, which to my mind overstates the role of Rousseau, underplays the importance of context, and is too insistent generally on problems. But I hasten to add that such judgments are hasty comments on highly complex and skilled works of history. With more discussion and debate, these new riches will become easier to manage.

³⁷ George V. Taylor, "Revolutionary and Nonrevolutionary Content in the *Cahiers* of 1789: An Interim Report," *French Historical Studies*, 7 (1972): 479–502; Roger Chartier, "From Words to Texts: The *Cahiers de doléances* of 1789," in Roger Chartier, *The Cultural Uses of Print in Early Modern France*, trans. Lydia Cochrane (Princeton, N.J., 1987), 110–44; and Timothy Tackett, "Nobles and Third Estate in the Revolutionary Dynamic of the National Assembly," *AHR*, 94 (1989): 271–301.

One Good Turn Deserves Another:
Moving beyond the Linguistic;
A Response to David Harlan

JOYCE APPLEBY

AFTER HISTORIANS MADE THAT LAST TURN MARKED “linguistic,” they ran into some dangerous curves. Scholarly vehicles were totaled; avenues of inquiry left in disrepair. The timid got out their maps to look for alternative routes to the past; die-hards demanded that the dividers be repainted. Some who managed to drive beyond the curves recommended ditching the cars for buses. Fueled by renewable verbal meanings, these buses, they said, add *jouissance* to the trip, even if they never take you where you want to go. David Harlan falls into this last group. Forget the archival loneliness of reconstructing the past, he advises, and fall into a conversation with a dead author.¹ Harlan’s witty exposition of the linguistic turn and its implications for historians invites engagement.

If I may recapitulate Harlan’s argument, it goes something like this: the deconstructionist critique of language has exposed a rupture between signifier and signified, leaving the signs that once stood for their union, that is, words, free to change meaning independent of the word users’ intentions. Among those word users, historians are hit hard. They rely on the stability of word meanings at two points: when they write their interpretations of the past and when they read the texts that serve as evidence of the past. Thus, for historians, the linguistic turn has precipitated an epistemological crisis. Without the bond between signifier and signified, they have no secure language for writing history and no recoverable references in the texts they scrutinize in order to reconstruct the past.

As Harlan readily admits, a long line of skeptics—most of them writing in English—have preceded deconstructionists onto the terrain of doubt. Carl A. Becker questioned the notion of a fixed and knowable past in his AHA presidential address of 1932. William James lacked only the word “repristinate” to describe the dubiety of locating the original context of human thought. Even James Madison remarked on the elusiveness of language. In one of the wittiest passages in the *Federalist Papers*, he wrote, “When the Almighty himself conde-

¹ David Harlan, “Intellectual History and the Return of Literature,” *AHR*, 94 (June 1989): 581–609.

scends to address mankind in their own language, his meaning, luminous as it must be, is rendered dim and doubtful by the cloudy medium through which it is communicated.”² If these wry reflections about our ability to render intelligible the meaning of others have been around for so long, why have the poststructuralists provoked such anguish in the groves of academe?

Harlan’s answer, set forth in some detail, maintains that the poststructuralists have eclipsed the waxing influence of another group of language interpreters, the contextualists. These contextualists, most prominently Quentin Skinner and J. G. A. Pocock, saw in language the entry into a past conceptual universe. Self-conscious helmsmen, they steered an entire generation of scholars to their destination of recovering the meaning of historical texts through the reconstitution of authors’ intentions. The contextualists regarded reconstructing the past “as it actually happened” impermissibly empirical, but they held out hope that meaning might be salvaged from the ravages of time if historians recognized that language was locked into time and place by specific usage. Because of this localization of meaning, they concluded that texts were incapable of moving beyond their particular voices to become part of a transhistorical tradition of canonical works on great themes of Western civilization. Alternatively, the context of social structure was mute about the linguistic means available to historical actors. Class identity offered too few clues about the word games dominant at any one time and place. Informed by this understanding of language encoded through experience, contextualists charted an exciting new scholarly course that avoided the Macphersonian Scylla of Marxist materialism and the Lovejoyean Charybdis of idealist history.

Suggesting that nothing is quite so embarrassing as fighting a rearguard action from the van, Harlan explains how the deconstructionists—Jacques Derrida, Michel Foucault, and Paul de Man, with an assist from Hans Georg Gadamer—have routed the contextualists. With ill-concealed delight, he details the undoing of the contextualists in just a few skirmishes, most of them in France. It started with the “death of the author.” Then, like modern terrorism, the attack continued through disappearances: “the vanishing text,” followed by the knowing subject, the historical agent, the authorial presence, and finally the network of intellectual discourse. Words have become hostage to a new philosophy of language, Harlan tells us, and are no longer available as the building blocks of history.

Harlan does not lament these losses; he yearns for an older form of intellectual history that the contextualists unceremoniously dismissed as naïvely presentist. With an inadvertent assist from the poststructuralists, Harlan can now make his case for interrogating texts for their possible contribution to the present, regardless of their historical origins. Rather than probe for the writer’s intentions, the new ahistorical historian strips old texts of arcane references and outdated foolishness. He or she “reeducates” the ancient author while simultaneously rendering the relevant residue accessible to contemporary readers.

² Alexander Hamilton, James Madison, and John Jay, *The Federalist Papers* (1787–88; New York, 1937), 230 (no. 37).

The epistemological crisis has clearly become an invitation to postmodernism for Harlan. If historians yielded up their claim to a monopoly on the past, they could do business with a host of contemporaries interested in narrativity. Disburdened of their task of re-creating old discursive practices, they could freely endorse presentism along with indeterminacy, relativism, and essential absences. Thus unencumbered with disciplinary fixations, they could join literary theorists, deconstructionists, and other liberated readers on a *fin-de-siècle* romp into the next century.

TO ALL OF THIS, I SAY AMEN. If we cannot have any theory of knowledge, much less a unified one, let us besport ourselves among the plethora of intellectual delights that knowing subjects can create. But, before we eliminate contextualism as one of them, the grounds for dismissal should be revisited. Harlan writes as though the poststructuralists have delivered a knock-out punch. By presenting this *fait accompli* in the form of a report on the poststructuralist critique of language, his proof comes to us as a set of assertions, but, happily, ones that we can easily rebut.

The lynchpin of the poststructuralists' argument is that words are no longer captive of the system we call language. Unchained from a fixed referent, words merely point to other words in "the incessant and unremitting play of signifiers." We are told that words form "an endless chain of signifiers in which meaning is always deferred and finally absent." Words, Harlan summarizes, are "protean and uncontrollable."³ Thoroughly anthropomorphized, words do appear a bit unstable, if not actually giddy. However, to speak of words as being out of control, freed from tyranny, chained to one another, is, if you will excuse the expression, meaningless. Words are totally inert. If they change meanings, it is because some sentient human being has embedded them in a new context that another human being has discerned. Whatever happens to words happens through the imaginative processes of their human inventors and users. Words are protean because human beings use them to explain, encode, describe, mask, obscure, convince, obfuscate, deny, exclude, abbreviate, express, reveal, and tease.

It was exactly this range of human capacities that informed the contextualists' intellectual mandate of the 1960s. Aware of the diversity of motives animating word users, they decided that the intentions of authors offered a better guide to the historical meaning of a text than the interests of their class or the relation of the text to some imagined transhistorical discourse. Skinner made the point emphatically: any statement is "inescapably the embodiment of a particular intention, on a particular occasion, addressed to the solution of a particular problem, and thus specific to its situation."⁴ The contextualists further asserted, and attempted to demonstrate in their scholarship, that texts were part of a

³ Harlan, "Intellectual History and the Return of Literature," 582.

⁴ Quentin Skinner, "Meaning and Understanding in the History of Ideas," *History and Theory*, 8 (1969): 50.

socially created discourse. The intentions of authors were directed and constrained by the authors' conceptual universe as it in turn was constituted by ideological assumptions, rhetorical strategies, and discursive conventions.

The contextualists' enterprise, worked out principally in the 1970s, converged with parallel undertakings in cultural anthropology, the sociology of knowledge, and the history of science. For scholars in all these fields, the concept of ideology, referring to socially structured systems of meaning, replaced the more rational and individualistic term, "intellectual." Contextualists discovered that the paradigms of social thought giving meaning to words had changed so decisively that historians, like archaeologists, had to dig for past settings of discourse. As Clifford Geertz explained, writing in that innocent pre-poststructuralist time of "Ideology as a Cultural System," the sociology of knowledge ought to be called "the sociology of meaning, for what is socially determined is not the nature of conception but the vehicles of conception."⁵ While deconstructionists do not challenge this assertion, they add the caveat deducible from Gadamer's work that, in setting out to determine social meaning and convey our findings to others, we are constituting a new reality, not reflecting a past one. Animated by our own passionate prejudices, we simply add our link to the chain of textual interpretations.

Harlan introduces Gadamer as the author of "a devastating critique" of the hermeneutical project, but Gadamer's argument is a double-edged sword. Gadamer said that historians are embedded in their own historical traditions ("History does not belong to us; we belong to it") and further that the texts they read are themselves a part of an interpretive tradition.⁶ This image of successive interpretations, however, is scarcely distinguishable from the successive paradigms in political discourse studied by the contextualists. When Pocock set out to capture the "Machiavellian moment" in England, he was reconstructing one of the many traditions inspired by Machiavelli's writings. Indeed, the idea of interpretive traditions undercuts the claim that words are uncontrollable. Repetition and communication form the essence of a tradition, and neither are assimilable to the notion of protean words dancing away with meaning before the author's ink can dry on the page.

Specific genres generate expectations in readers simply because of the stability of form, of rhetoric, of emplotment. Indeed, to say, as Harlan has Gadamer saying, that we can never recover the tradition in which a text was written but only the tradition of interpretation that has grown up around it raises the logical point of why one recovery is possible and not the other. If we can talk about traditions, why can't we talk about the norms and conventions that give stability to language? If it is not the union between signifier and signified that establishes discursive practices, where are we to look for the structuring force?

A similar out-the-front-door-in-the-back-door maneuver attends the dethroning of authorial authority. After treating his readers to the harrowing orbits of

⁵ Clifford Geertz, "Ideology as a Cultural System," in David Apter, ed., *Ideology and Discontent* (Glencoe, Ill., 1964), 59.

⁶ Harlan, "Intellectual History and the Return of Literature," 587–88.

intertextuality, Harlan makes a soft landing by declaring that no one—not even Roland Barthes—“in actual fact” has any trouble telling the difference between great books and comic books.⁷ While this is an enormous relief to hear, it does throw us back into the domain of reasons, norms, and stable meanings, whose absence had created the epistemological crisis in the first place.

In this, as in many places in Harlan’s essay, alternatives are over-dichotomized. This resembles the logic of an all-or-nothing approach to issues of proof. Harlan’s observation that we cannot approach the past “in a state of historical virginity” because of the passionate prejudices that make us human is used to undermine the entire enterprise of re-creating historical contexts. Far better, I would say, to abandon the notion that we can render our minds into *tabulae rasae*. Elsewhere, Harlan tells us that it is difficult “to continue approaching our texts as objects that *should* be transparent” or to yearn for an encounter with the “now-dead authors in the body of their texts.”⁸ It is a tribute to Harlan’s verbal dexterity that the straw men littering his pages only become conspicuous when picked out of the text.

NONE OF THESE ISSUES IS TRIVIAL. The deconstructionists have issued a powerful challenge to that philosophical tradition that asserts the existence of objective truths, considers language a vehicle for the discovery and articulation of those truths, and depends on the stable passage of words from author to reader to spread them. No one reading Derrida or Richard Rorty or Foucault could fail to appreciate the seriousness of the effort to depose this reigning epistemological tradition. The importance of their work, exhilarating, liberating, and cautionary, cannot be exaggerated. However, there is insufficient agreement among these thinkers to undermine our confidence in communication.

Let us consider the question of authorial intention. All of us who write know that we are animated by intentions and further that our intentions, once encapsulated in language, will be comprehended, distorted, elaborated on, and cannibalized by readers. The presence of a vital exchange between author and reader does not eliminate authorial intention, nor does it eliminate curiosity about what those intentions might have been. Similarly, all of us who write know that every other text known to us is a resource—acknowledged or unacknowledged—in our writing. Why should these reflections lead ineluctably to the proposition that intertextuality causes an endless deferment of meaning? Rather, it seems to me, present meaning is not deferred; it stops with every satisfied reader. Only meanings that others in the future might find can be described as deferred.

No logical argument is presented by Harlan, nor have I offered one for my counter-assertions. I appeal to experience. And herein lies one of the problems of proof for historians. Physical scientists externalize their validation process

⁷ Harlan, “Intellectual History and the Return of Literature,” 597.

⁸ Harlan, “Intellectual History and the Return of Literature,” 588, 592, 602.

through experimentation and demonstration. Many social scientists imitate them by attempting to reduce their investigations to those elements in social life that can be externalized and measured. Humanists cannot follow this path; their validating process involves the assent of a knowing subject. And the knowing subject lives the judgment rather than finds it. Let us take Harlan's claim that "language is an autonomous play of unintended transformations."⁹ If I assert in contradiction that only intentional human beings play with words, I must appeal to common experience for proof. Like Barthes's discrimination between great books and comic books, other historians will agree with my statement, but this convergence will follow from our shared practice of doing history at a particular time and place rather than from our commitment to a set of standards abstracted from experience. Can the process through which humanists arrive at common judgments be more adequately explained? If not, is it any the less valid because of its dependence on shared participation in a complex intellectual practice? Perhaps one should not be surprised that lurking beneath battles over words, meanings, and intentions is the archetypal opposition of free will and determinism. By anthropomorphizing words and giving them wings to fly away from human beings, the deconstructionists have created a new set of social forces that impose themselves on human beings. "Words speak people." Turning words into protean, self-animating forces permits us to disregard the troubling issue of human agency. The complex ways that human beings initiate actions, comply with conventions, and dissent from norms can be ingeniously ignored if surrogate forces such as discourse, class, and culture are called on to explain events.

There are really two issues about determinism involved here. One uses the frustration of particular intentions to argue for the insignificance of human intentionality. The claim is, because I cannot control all of the meanings that readers will find in my text, my acted-on intention to write this text is not a causal force. The other stems from a confusion about what causes change. Foucault's claim that discursive practices are subject to abrupt ruptures has encouraged the deduction that changes are independent of human agency, prompted, if at all, by distant, unspecified powers. Yet can anyone doubt that change needs human speakers and writers to introduce novelty and compliant language users to give currency to discursive innovations?

With imagination and opportunity, any collectivity—practitioners of a calling, members of a club, celebrated media figures—can start a fresh language game or shift into a different metaphorical gear. Human beings can even jettison old metaphysical problems, as Rorty has recommended, by ditching the discourse in which those metaphysical problems reside.¹⁰ Innovation need not be a solitary accomplishment in order for human agency to figure in its genesis. Social practices usually change when groups—often cohorts—change, but the plurality of persons does not take away the distinctively human, specifically intentional, character of their action. The question then arises (the inevitable regression

⁹ Harlan, "Intellectual History and the Return of Literature," 596.

¹⁰ Richard Rorty, "The Contingency of Language," *London Review of Books*, 8 (1986): 3–6.

behind language), how are we to explain when others fail to respond and thus abort a discursive initiative? Engagement with this question will throw us back onto the terrain outside of language, that area that deconstructionists say does not exist and materialists claim as fundamental.

Language, purpose, power, free choice, determinism—these are the heady words, redolent with meaning and brimming with evocative power that we smuggle into codes and embed in myths. The twenty-first century beckons, and we struggle to respond to its millennial openness by taking stock of our experience. In the beginning, there was the word; in the end, there is tangled intertextuality. The library of human chatter is vast; meanings have been catalogued; expressions checked out and lost. True enough, but do these observations justify eliminating the intending author and the knowing reader from our interpretive quiver? In this endless intertextuality lies the record of human beings talking to and with and behind the backs of one another. We remember but a fraction of it; we must recover all else. Of course, we live and think in the here and now; the question is whether we can re-create any part of the past to keep us company. If the poststructuralists are correct that we cannot fathom the original meaning of the texts offering us a window on other human experience, we will remain imprisoned in the present. Small wonder that historians draw upon their practice of reconstructing the past in order to resist this verdict.

Reviews of Books

GENERAL

ROBERT PURKS MACCUBBIN, editor. *'Tis Nature's Fault: Unauthorized Sexuality during the Enlightenment*. New York: Cambridge University Press. 1987. Pp. 260. Cloth \$34.50, paper \$10.95.

This volume is a collection of essays by American and European historians and literary critics dealing with "unauthorized sexuality," that is, sexual acts defined as sinful, unnatural, or both, in the eighteenth century. Most concern England and France. As is logical in a collaboration between historians and literary analysts, both the historical realities of sexual license and the discourses about it are covered.

The essays fall into four groups. One concerns authorized sexuality, that is, heterosexual relationships within marriage. Two others include two important types of unauthorized sexuality, male homosexuality and male libertinism. The final category of articles deals with discourses on sex and includes a number of essays by literary historians on various aspects of pornography. Unless their tastes run that way, historians will probably find these articles the least useful. They cover only the literary aspects of the subject and show little awareness of its historical dimension—for example, the link between pornography and political subversion explored by Robert Darnton. Indeed, the tendency of the contributors to address only those within their own discipline is one of the disappointments of this supposedly interdisciplinary collection. At a time when historians are increasingly borrowing the techniques of literary criticism, one wishes for more collaborative efforts.

Historians will, however, find useful and impressive the work of their colleagues doing their own thing. Roy Porter analyzes England's most popular sex manual to argue convincingly that in the eighteenth century most people thought of sex in terms of reproduction, not individual pleasure; the manual's major topic is how to beget children, especially male heirs. John Gillis explores the

sexual habits of the English lower classes, showing that, while their informal marriages, cohabitation, and illegitimacy appeared sinful to the elite, they were normal and acceptable within lower-class communities. And Randolph Trumbach provides an excellent survey of the recent historical work on male homosexuality. Indeed, the four articles on homosexuality—Trumbach's, two case studies drawn from police records on the homosexual subcultures of Paris and Holland, and George Rousseau's prize-winning essay on homosexuality in English literature—are the book's highlights. It is increasingly clear that the eighteenth century was a watershed in the history of homosexuality, a period that saw the final emergence of distinctive homosexual subcultures in Europe's major cities and the first perceptions of the homosexual as a unique personality type. The articles illuminate those trends and make the book required reading for scholars interested in the topic.

The collection is less useful, however, for those interested in broader aspects of the history of sexuality, because in general it deals only with the sexual experiences of upper-class men. Only Gillis covers the lower classes. Only Vern Bullough's study of London's prostitutes deals with women, and it is based on literary sources written by elite males. To be fair, the editor himself laments the book's lack of a "feminist"—I would say merely "feminine"—perspective. But awareness of a fault does not excuse it. Why are there no articles on illegitimacy, whose rise is one of the striking trends of the history of sex in the period? Why are there no case studies based on police records of the subculture of prostitution? Why nothing on lesbianism in the age of the Ladies of Llangollen? A feminine perspective would have been useful not just for completeness but also for clarification of those topics the book does cover. Our understanding of pornography would be deepened by a study of its depiction of women and our understanding of homosexuality by a look at the new eighteenth-century definitions of femininity. Surely the his-

tory of sex is one topic where historians cannot ignore gender.

CISSIE FAIRCHILD
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PIERO CAMPORESI. *The Incorruptible Flesh: Bodily Mutation and Mortification in Religion and Folklore*. Translated by TANIA CROFT-MURRAY. (Cambridge Studies in Oral and Literate Culture, number 17.) New York: Cambridge University Press. 1988. Pp. ix, 286. \$44.50.

Piero Camporesi has published seven books in the last ten years, three of them on food and three on the flesh. As Peter Burke says, in an introduction by no means entirely enthusiastic, this work is intuitive, impressionistic, and impossible to summarize. Camporesi seems to argue—insofar as such fluid and extravagant prose can be said to “argue”—that life in medieval and early modern Europe had a somatic quality that we have lost since the emergence of modern biology and the germ theory of disease. The body, in earlier European history, was the locus of rottenness and therefore of fertility. Fear of spontaneous generation led people to view all flesh as seething with life. Therefore, paradise was understood to be an intense bodily experience of sweet smells and unchanging beauty, and the holy, because they were holy, were said to inhabit impassible and incorruptible bodies.

Traditional historians will find this book offensive, both in substance and in method. They will question whether so many descriptions of mutilation and excrement are necessary. They will find Camporesi's cavalier treatment of chronology and of conventional standards of proof disquieting. They will query whether one should take seriously a discussion of “the Flesh of God” that sees early modern mysticism as mushroom-induced hallucination but ignores the Christian eucharist.

Yet it would be unfortunate if Camporesi's excesses led to dismissal of his work or to the denigration of important research on the history of the body now being done by younger scholars (both European and American). Careless though this book may be, it is also brilliant and based on wide reading. Its evocation of cultural attitudes through lengthy quotations from medical texts, imaginative literature, and folk material supplants the unconvincing picture given by those intellectual historians who assume that behavior and belief are governed by the law of noncontradiction. Camporesi is at home with paradox. It is true that he at times equates decay too closely with death and neglects the importance of fertility. But his insight into conceptions of the body is pro-

found. To Europeans between the twelfth and the eighteenth centuries, the body was a center of rotting and swarming life, and escape from it was conceptualized not as the soaring aloft of immortal spirit but as the achieving of incorruptible flesh.

CAROLINE WALKER BYNUM
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EDWARD PETERS. *Inquisition*. New York: Free Press. 1988. Pp. vi, 362. \$24.95.

Historians have long known that “the Inquisition” never existed; Edward Peters asks why the educated public continues to believe in it. Like the author's earlier works on heresy and torture, this study is a learned, coherent history of an important concept. Not footnoted but with an annotated bibliography, this clearly organized book draws on a wide range of historiography, theology, art history, etymology, literary history, even musicology.

The first part of the book briefly summarizes what is known about the word and concept of *inquisitio* from pagan Rome to the present. The interplay of concepts and words is always difficult, and the author never quite solves the problem of distinguishing between investigation, inquest, inquisitorial procedures, and inquisition. Peters describes critical junctures in the history of the concept: the linkage of heresy with treason by the Christian emperors, the revival of inquisitorial procedures in the eleventh century by temporal and spiritual powers, and the transformation of religious dissent into a specific crime of heresy by the canon lawyers. Inquisitions against heresy existed from the time of the Council of Verona in 1184, and a papally directed inquisition against heresies using the mendicants may be said to have begun with Gregory IX in 1231. In the late fourteenth and early fifteenth centuries, the papal Inquisition, never more than vaguely organized, declined because of the increased efficiency of temporal and episcopal courts. The Spanish Inquisition, aimed primarily at Judaizers, was founded as a council of state in 1483 and abolished in 1834. The Roman Inquisition was revived in 1542; its name was changed in 1908 to the Congregation of the Holy Office and in 1965 to the Sacred Congregation for the Doctrine of the Faith. The revived Roman Inquisition was localized and seldom had power outside Italy until the nineteenth century; I would have liked more discussion about how it was turned by the modern papacy into a universal (though seldom terrifying) agency for orthodoxy.

The second part of the book is a history of the idea of “the Inquisition,” that familiar, nonexistent, international, conspiratorial institution sym-

bolized by hooded men, torture chambers, sadism, greed, corruption, and superstition. Peters describes how "the Inquisition" was invented by sixteenth-century Protestants as a club against the Catholic church and then expanded by the Enlightenment, which used it as a club against Christianity. "The Inquisition attacked by the Enlightenment, symbolizing the conspiracy of implacable and irrational tyrants and popes to block the emergence of liberty, bore little resemblance to actual practice." Peters naively contrasts myth (bad) with history (good), but his point is taken.

In literature, from Voltaire's *Candide* through "Monk" Lewis, Johann Schiller, and Fedor Dostoevsky to the tongue-in-cheek Umberto Eco, in art such as Goya's works, in Verdi's operas, "the Inquisition" is the focus of evil in the world. The first serious, critical history of the Spanish Inquisition was written by Juan Antonio Llorente early in the nineteenth century, using the archives that Napoleon opened up. Llorente and most other nineteenth-century historians, including Henry Charles Lea, shared the Enlightenment view. Peters, H. C. Lea Professor of History at the University of Pennsylvania, rightly names Lea as the greatest historian of the subject, but Lea shares with John W. Draper and Andrew Dickson White some responsibility for perpetuating the stereotype. Only in the past fifty years have historians from Herbert Grundmann to Richard Kieckhefer thoroughly dispelled among scholars the illusion that "the Inquisition" ever existed.

Peters's last chapter on the common twentieth-century use of "the Inquisition" as a metaphor for anything oppressive illustrates how words become meaningless through overuse. When McCarthyism is seen as a manifestation of "the Inquisition," it is time to find new words, new metaphors. Peters's work is a powerful tool in abolishing "the Inquisition," but popular lies die hard.

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ALEX CALLINICOS. *Making History: Agency, Structure, and Change in Social Theory*. Ithaca: Cornell University Press. 1988. Pp. viii, 275. \$39.95.

Alex Callinicos explores the complex relation in Marxist theory of "structural explanation" to "intentional understanding." This is not the same distinction as that between society and individual or between causal (nomothetic) theory and interpretive (ideographic) theory or between structuralism and phenomenology, but the difference is close. Taking his cue from the work of Anthony Giddens, Callinicos is concerned with the relation

of "structure" to "agency." His thesis is that "structure and agency are so closely interwoven that to separate either and give it primacy over the other is a fundamental error" (pp. 6-7). This position smacks of the reasonable and commonsense world of British and American thought and scholarship. Only theoretical fanatics with some peculiar axe to grind would be moved to deny it. Indeed, Callinicos acknowledges his debt to the tradition of Anglo-American analytic philosophy, particularly as practiced in recent years on the corpus of Marxist writings. He provides the reader with an outstanding survey of the rich body of literature that has emerged since the 1960s on Marxist theoretical issues. The range of his expertise in this area is broad, stretching from social theory (for example, Anthony Giddens, Eric Olin Wright) to history (for example, Robert Brenner, Perry Anderson), but it is strongest in philosophy itself (for example, G. A. Cohen, Jon Elster, John Roemer).

Historians may or may not be interested in accompanying Callinicos on his philosophical tour of the dilemmas of Marxist theory. In particular, the first three chapters of this book may appear irrelevant or arcane to historians who have never thought much about the relation of structure to agency. The effort, however, would be well rewarded because Callinicos—in clear, straightforward prose, the hallmark of analytic philosophy—discusses with great subtlety issues that are at the heart of historical writing. The remaining two chapters concern issues that are more easily recognized by historians as being within their purview. In the author's words, these chapters "explore in more concrete terms how human beings draw on the powers they derive from their position in the relations of production to sustain, but also to transform societies" (p. 6).

Callinicos seeks to follow a path between the dangers of structural determinism and subjective idealism. There are many variations of each of these dangers, and Marxists will readily recognize the issue as that between determinists and voluntarists. One position that claims largely to have overcome this dilemma is poststructuralism, whether in its Foucauldian or Derridian variants. Callinicos proves to be seriously weak in his understanding of poststructuralism. For instance, his discussion of Michel Foucault dismisses him for having abolished the subject. Although it is true that for a brief period in the late 1960s and early 1970s writers such as Louis Althusser and Foucault, then much taken with structuralist theory, tended to reject the notion of the subject, tainted as it was by "philosophies of consciousness," poststructuralist writing is not characterized by such a radical position. Instead, Foucault and

others have centered their recent attention precisely on the way the subject is constituted in discourse and practice.

Callinicos has little sympathy or understanding of this body of literature. He relies on the work of Giddens and Anglo-American analytic philosophy to affirm the role of the subject in collective action. He favors a particular view of individuals based on the concept of interest. In order to support a notion of agency that is effected but not determined by structure, Callinicos defends a notion of interest that inevitably relies on a form of Cartesian rationalism. The "interest" of workers becomes, as it has in the past, their recognition of the rationality of collective action and socialist revolution. But such a theoretical turn, one typical of Marxism, avoids the question of the actual constitution of very nonrevolutionary working-class subjectivity in advanced industrial society. In addition, it places the theorist and the historian in the position of defending male, patriarchal definitions of the subject, definitions that bypass the problem of the forms of subjectivity of women and ethnic or racial minorities. If historians are squarely to face these issues, they must turn to the problem of cultural formation that has been best explored not by Marxist theorists but by Foucault and feminists.

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B. A. HADDOCK. *Vico's Political Thought*. Swansea, U.K.: Mortlake. 1986. Pp. 238. £25.00.

Every scholar of Giambattista Vico must at some time or other confront the issue of anachronism. It was Vico's own preoccupation to defeat the notion that the near-brute minds of our earliest ancestors could have held, even poetically, the refined ideas of later philosophers, and his interpreters in turn have come to put their own question of anachronism, some finding others too willing to see in Vico's endlessly challenging (and frequently ambiguous) text the theoretical innovations of a much later day.

Anachronism in both of these forms is the leitmotif of this admirable new book by B. A. Haddock. In earlier work Haddock explored the two forms separately, addressing such themes as Vico's Homeric research and his place in the development of modern historiography. Here Haddock combines those interests, concerned to show how Vico's reformulation of traditional rhetorical and jurisprudential materials yielded a new science of history and society that challenged the scope and method of conventional political thought. The targets of critique are largely the

natural law and social contract theorists of the seventeenth century—Grotius, Hobbes, Spinoza, and Locke. But in some closely argued final pages Haddock brings under Vico's assault any and every effort to warrant political obligation through language ("nature," "contract," "utility," whatever) that is dissociated from the society that produces it. Thus, even David Hume's utilitarianism, which Haddock finds otherwise compatible with Vico's ideas, comes up short. After Vico, Haddock concludes, the task of political philosophy is no longer to furnish timeless principles of obligation but to depict the institutions and arrangements that have satisfied the needs of human beings at different stages of their development.

As limited as its intentions are, in performance the book is yet bolder, being a careful and intelligent reading of the entire Viconian corpus, including some of its minor or neglected works. Thus, in successive chapters on rhetoric, the "earthly city," law, and society, the progress of Vico's thought is traced from an early preoccupation with practical life in his youthful school orations to the developed historicism of his final *New Science*. So wide an agenda causes an occasional loss of focus, and one sometimes wonders whether a line of inquiry is opened simply for the pleasure it gives the author. Yet the pleasure is not wholly his, for some of these excursions—for example, the Augustinian and Tacitean strains in Vico's early work—are among the book's most thoughtful passages.

Perhaps from this need to cover so many bases, however, Haddock stops short of probing as deeply as he might the themes in political thought that are his book's central concerns. This extends to his principal argument that Vico's extreme historicism invalidates prescriptive talk, language that is cut off from the institutions that embody it. In describing Vico's scientific breakthrough, he speaks of Vico's science as a "framework" that "delimit[s] the range of meanings that could be attributed to myths, traditions or texts" (p. 134) and later adds that for Vico "institutions provided a degree of regularity that makes generalizations about human affairs possible" (p. 172). Applied to politics, such generalizations give no particular standing to ideals or values, whose relevance is always temporary (p. 200), but merely ensure that certain ideas will always be bound to certain institutions and will always unfold in a certain sequence. But, if this is truly the extent of Vico's thinking, why should he have rejoiced in the progressive idealization of law, in the movement of human beings from coarse and cruel ways to a state of natural equity, and why should the return to barbarism, however necessary and cathartic it might be, be accompanied by such a sense of loss?

Either these are archaic gestures not to be confused with Vico's memorable insights, or they are signs that Haddock has given us a lucid and penetrating but slightly anachronistic rendering of Vico.

MICHAEL MOONEY
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M. E. MOSS. *Benedetto Croce Reconsidered: Truth and Error in Theories of Art, Literature, and History*. Foreword by MAURICE MANDELBAUM. Hanover, N.H.: University Press of New England. 1987. Pp. xii, 150. \$20.00.

By paraphrasing Benedetto Croce's 1906 question regarding Hegel, we may ask: "What is living and what is dead in the philosophy of Croce?" Croce's activity encompassed the first half of our century and influenced generations of intellectuals, even Marxists, in and outside Italy. As a student of Antonio Labriola, he retained certain elements and themes of Marxism and built them into his system. Antonio Gramsci criticized and Palmiro Togliatti admired him. Croce's practical-political activities included being twice minister of culture (1920 and 1944). After an initial confusion, from 1925 on he opposed Benito Mussolini's Fascism. His theoretical output amounts to over sixty volumes, covering areas of history, philosophy of history, esthetics, and Marxist theory.

M. E. Moss's main concern is not with Croce's political activities, nor with his philosophy of the practical sphere of human endeavors—the economic, political, and ethical realms—but with his "philosophical conceptions of truth, errors and objectivity, the realm where Croce made major contributions to contemporary political thought" (p. 19). In the chapter titled "The Sphere of Aesthetics," Moss traces the evolution of Croce's theory of intuition as stimulated by the reciprocal reactions among his activity as literary critic, historian, and philosopher. In his rejection of nineteenth-century positivist historiography, Croce proposed that the study of the past be included under the concept of art and intuition of particular objects. Along with his German contemporaries (Wilhelm Dilthey and Wilhelm Windelband), Croce argued that "historical narrative could not form a science composed of empirical laws, concepts and their relations. It was to describe instead particulars in their concreteness" (p. 39). Croce's reductionism to intuition seems to be a narrow approach. By traditional standards, aesthetics can be approached from the stand point of the creator and of the receiver and by looking at the "work" itself. By standards of modern and postmodern aesthetics, he is even less "living."

Moss did a good job within her self-imposed limitations, but the real "revisiting" of Croce to give a definitive answer to the question of what is living and what is dead in his philosophy is still outstanding. It would require an analysis of historical-sociological conditions that engendered Croce's system and its roots in the humanist national tradition of Italy as well as its place in relation to the twentieth-century international philosophical movements. This approach would clarify to what extent Croce's system is bound by time and place and "what is living" beyond this existential conditioning. Croce was troubled by Italian provincialism yet contributed to it by excluding from his system themes that became central to twentieth-century philosophy, such as the philosophy of science, and by ignoring thinkers of significance like Sigmund Freud, Ludwig Wittgenstein, and György Lukács.

I hope that Moss's study will stimulate interest in Croce's philosophy, which dominated the Italian intellectual scene in the first half of the twentieth century, was eclipsed after his death in 1952, and seems to be gaining recognition in light of our postpositivist philosophical era. Croce's ideas about theory and methodology of history are an integral part of a tradition of scholarship from which everyone who is laboring in the founding of a nonpositivist or postpositivist humanistic and historical scholarship may benefit.

ZOLTÁN TARR
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DAVID WOODWARD, editor. *Art and Cartography: Six Historical Essays*. (The Kenneth Nebenzahl, Jr., Lectures in the History of Cartography.) Chicago: University of Chicago Press. 1987. Pp. xvi, 249. \$65.00.

An unusual combination of the varied interests of cartographers and art historians is represented in this collection of essays. The essays can be divided into three major groups according to the approach taken to the general theme of art and cartography: two essays examine the history of artistic techniques in cartography; another two deal with the history of cartography as art history; and the last two reflect on the nature of art and cartography.

The chapter by Ulla Erensvärd examines some of the symbolic meanings represented by colors among the Greeks, Romans, and Hebrews; origins of those meanings in the cultures of Babylon and central Asia; and the use of such symbolic color in maps by cartographers in Christian Europe. She then summarizes the highlights in the history of the use of color on Western maps from hand

painting through lithography and from illumination to geological mapping. David Woodward provides a history of map lettering that encompasses manuscript calligraphy, hand engraving, and typography. His essay ranges from the Carolingian minuscule to phototypography.

The essays by Juergen Schulz on the mural map cycles of the Italian Renaissance and by James Welu on the sources and development of cartographic ornamentation in the Netherlands are classical history of art studies focused on a particular artistic genre. Schulz considers four examples of late sixteenth-century mural maps painted to decorate rooms in Italian palazzos. He describes and illustrates the extent to which the murals were value-laden images designed to carry particular meanings, rather than exacting depictions of terrain. Welu looks at the ornamental detail on some of the best-known Dutch and Flemish maps, sea charts, and globes of the sixteenth century and traces some details to earlier and frequently non-cartographic engravings and etchings.

The chapters by Samuel Edgerton and Svetlana Alpers are thought pieces that, through the exploration of a particular theme, seek to cast light on the nature of both art and cartography. Edgerton seizes on what is commonly recognized as the fundamental characteristic of maps and mapping, the geometric sense of order represented by the grid. He identifies its presence in both art and cartography from the earliest times and in diverse cultures and links the rediscovery of Ptolemy's projections to the Renaissance development of perspective drawing. In an original and intriguing article, Alpers ably demonstrates the lack of distinction between seventeenth-century mapping and landscape painting in the Netherlands. She sees it as a consequence of the fundamentally graphic nature of Western geography since Ptolemy, the seventeenth-century Dutch conception of drawing as description, the shared field-based training of the era's painters and topographers, and their common approach to the recounting of events.

The book is handsome and well illustrated, as those familiar with Woodward's publications will no doubt expect. It does not claim, nor does it offer, a unified approach to the topic or a rigorous investigation of any particular theme. Rather, the collective effect of the essays is to suggest to the reader the many avenues of research open in the vast and unmaped region between art and cartography.

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GERALD HOLTON. *The Advancement of Science, and Its Burdens: The Jefferson Lecture and Other Essays*. New York: Cambridge University Press. 1986. Pp. xi, 351. Cloth \$39.50, paper \$12.95.

This collection of Gerald Holton's recent studies on the roles of science and scientists in our culture, past and present, exhibits the variety and richness that characterized its two predecessors, *Thematic Origins of Scientific Thought: Kepler to Einstein* (1973) and *The Scientific Imagination: Case Studies* (1978). Throughout his career Holton has been striving to meet "the great challenge" that he formulated many years ago: "to restore science to reciprocal contact with the concerns of most men—to bring science into an orbit about us instead of letting it escape from our intellectual tradition" (*Thematic Origins*, p. 159). The essays in this new collection exemplify some of the ways in which Holton has tried to deal with this central problem of our culture.

For over a quarter of a century, Holton has devoted a major part of his scholarly work to historical and philosophical studies on Albert Einstein, and Einstein is the focus of the first half-dozen essays in this volume. Several of these are concerned to set forth the underlying unity of Einstein's early papers, which seem to be about very different physical problems. Holton was the first historian to make this major point, to emphasize Einstein's search almost from the very beginning for a new and unified *Weltbild*. It is Einstein's philosophical attitudes toward science and their influence on his scientific work that Holton pursues through study of Einstein's correspondence and manuscripts as well as his scientific publications and popular writings. But Holton does not neglect Einstein's impact on twentieth-century literature and psychology, nor does he ignore the particular circumstances that brought Einstein to the already lively community of American physicists in the early 1930s. One need not agree with every point made in these essays to recognize them as significant contributions to our understanding of a genuinely heroic thinker.

Other papers in this volume are historical studies of more recent problems. One is a sketch for a comparative study of Werner Heisenberg and Robert Oppenheimer, a fascinating theme one hopes Holton will develop at greater length. Several studies attempt to characterize central features of science since the Second World War, particularly what Holton calls "Combined-Mode Research . . . where the imperatives of science and of society overlap instead of claiming mutual exclusivity" (p. 187). Holton's lifelong devotion to science education is evident in many places but especially in his study of *A Nation at Risk*, the 1983

report on education. As one of the eighteen members of the National Commission on Excellence in Education that prepared that report, Holton is in an excellent position to appraise the report and its reception, and his study makes sobering reading.

The book bears the title of its concluding selection, the Jefferson Lecture delivered by the author in May 1981. This is an impressive piece that ranges from the idea of the Jeffersonian Research Program—"basic scientific study which had no certain payoff in the short term but was targeted at an area of national importance" (p. 281)—to the equally Jeffersonian theme of the political dangers, the threat to our society, of a citizenry that lacks the knowledge to make informed decisions on issues having scientific or technical components. That Holton was the first scientist chosen by the National Endowment for the Humanities for what it considers "the highest honor the federal government confers for distinguished intellectual achievement in the humanities" (p. x) recognizes the unique position that he occupies in American academic life.

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J. V. FIELD. *Kepler's Geometrical Cosmology*. Chicago: University of Chicago Press. 1988. Pp. xx, 243. \$37.50.

The last several years have marked the end of a long period during which Johannes Kepler was not the subject of a book-length essay on his ideas, sources, calculations, or deductions. Unlike both Galileo and Isaac Newton, Kepler has been dealt with in journal articles or as one of several figures in the scientific revolution. Although all three of those outstanding pioneers of recognizably modern physical science are of nearly equal importance to its history, only Galileo and Newton have been studied as unique individuals in their own right.

In 1987 Bruce Stephenson's *Kepler's Physical Astronomy* set forth a more thorough analysis than ever before available of the concepts and astronomical calculations by which Kepler arrived at the three laws of planetary motions for which Newton provided valid theoretical demonstrations. The *Astronomia Nova* was given principal, indeed almost sole, attention among Kepler's books. In contrast, J. V. Field deals with the cosmological theories, astrological views, and various philosophical, theological, and musical considerations that dominated the thought of Kepler in matters related to, but not integral parts of, the science of astronomy. The books central to Field's

monograph are Kepler's *Mysterium Cosmographicum* (1596) and *Harmonices Mundi* of 1619.

It is entirely proper that two different phases of Kepler's thought and work have been dealt with separately by historians having quite different interests and purposes. The two books jointly provide a wealth of information about the German pioneer of modern astronomy, with hardly any duplication of effort. Each includes novel, and sometimes idiosyncratic, interpretations or reconstructions along with the accepted results of research by other specialists in the respective fields of astronomy and of cosmology (or, one might say, of science and of philosophy).

As to Kepler's philosophical outlook on cosmology, Field expands the considerations (long generally accepted) that mark Kepler as wholeheartedly Platonist. Curiously, he regarded the *Timaeus* as undoubtedly a commentary on the Book of Genesis, "transforming it into Pythagorean philosophy, as will be apparent to an attentive reader who compares it with Moses' own words" (p. 1). Thus, Kepler's cosmology was as deeply rooted in his theology as it was in his Platonist philosophy.

This citation from a remark in the *Harmonices Mundi*, not widely noticed by previous researchers, is given a prominent place in Field's first chapter, titled "Platonic Science," preliminary to his examination of the *Mysterium Cosmographicum*. The second chapter deals with the size of the universe and the status of the sun in it. After these background studies, the third chapter takes up the *Mysterium Cosmographicum*, fortunately accessible in full in English translation, and the fourth chapter examines the second edition published in 1621, in which Kepler took account of his discovery (1619) of the third law of planetary motions. The fifth chapter is devoted entirely to the *Harmonices Mundi*, with attention to the musical implications of Kepler's time that are included in it and to the importance of proportionalities in the geometrical cosmology of the German pioneer of modern astronomy.

The final chapter sets forth the conclusions drawn by Field from his specialized research and includes discussion of the Kepler-Robert Fludd controversy. Appendixes on the diameters of stars, the inscribed and circumscribed spheres for the cube, the radii of such circles for regular polygons, and Kepler's work on rhombic solids complete the book.

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ARTHUR FINE. *The Shaky Game: Einstein, Realism, and the Quantum Theory*. (Science and Its Conceptual

Foundations.) Chicago: University of Chicago Press. 1986. Pp. xi, 186. \$25.00.

Albert Einstein wrote to Erwin Schrödinger in 1950, "Most of them [the quantum theorists] simply do not see what a risky game [*gewagtes Spiel*] they are playing with reality." In this book the philosopher of science Arthur Fine analyzes, with admirable insights and documentation, a network of philosophical and historical issues connected with Einstein's so-called realist critique of quantum mechanics. Fine locates the focus of Einstein's distinctive and somewhat incoherent position in his insistence on an observer-independent realism—no more than that. Einstein's realist position is seen to be rooted not so much in cognitive and ontological claims as in a motivational attitude that guides scientists in the construction of a rational, lawfully determined science.

Fine has been working on the problem of Einstein's realist position for some time. Six of the book's nine chapters have been published previously. The integrated argument of the book falls short on continuity and readability but is peppered with brilliant insights and new interpretations of the debates between realists and antirealists surrounding three classic quantum problems: the Einstein-Podolsky-Rosen paradox (1935), the showpiece of Einstein's dissatisfaction with the quantum mechanical treatment of issues such as locality, completeness, and reality; Schrödinger's quantum cat paradox (1935), a conceptual or thought experiment device to portray the fuzziness of quantum observables; John Stewart Bell's inequality theorem (1981), a theorem regarding Einstein's statistical interpretation of quantum theory. Does Fine in his watered-down realist model really manage to avoid his own set of hidden variables?

In the second part of the volume, using Einstein's position as a malleable reference point, the author seeks to ditch the incurable difficulties of both the hard-core, dogmatic realism conventionally attributed to Einstein and the slippery, facile antirealism represented in the Copenhagen interpretation of quantum mechanics. Fine's way out is to reject both positions and to latch on to the "natural ontological attitude" (NOA). Historical scientific practice and precedent are championed in NOA and are seen to be the determinants for the particular historical mix of external realism (ontology) and inner human orientation (idealism, or David Hume's universe of the imagination) that enters into the various risky games that scientists have played and continue to play. One yearns for a comfortable grip on reality, but, alas, there are no rigid universal guidelines for the game. Physics is a perennially risky business. Nevertheless, Fine

argues that it is possible to achieve a minimal stance for an adequate philosophy of science that helps maintain a workable balance by adopting a realism that "adds an outer direction to NOA, that is, the external world and the correspondence relation of approximate truth," and an antirealism that will "add an inner direction, that is, human-oriented reductions of truth, or concepts, or explanations" (p. 133). Fine's counsel, ultimately, is pragmatic: if it works, do not question it.

As a philosopher of science with impressive expertise in physics, Fine has managed in this collection of essays to clarify various conceptual positions that recently have been advocated by partisans in the quantum versus realist controversies. The outcome of Fine's analysis, which draws on recent scholarship in the history of science, is that Einstein's realism did not embody a cognitive doctrine or set of specific beliefs about nature. Einstein's position, as Fine correctly argues, derives much from Ernst Mach and Hume. Fine concludes that classic realism is dead. Games of scientific practice take the place of games of truth.

The thrust of Fine's analysis bears resemblance to current attempts on the part of historians of science to redress the balance of historical analysis by exploring, with resolution, the reciprocity that maintains between theory and experiment or practice rather than concentrating so exclusively, as has been done hitherto, on scientific theory formulation. For most historians of science, the endless current philosophical debates on the merits and demerits of scientific realism are rather boring, unfocused, derivative, and historically uninformed. Those historians would do well to examine Fine's work with care.

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SPENCER R. WEART. *Nuclear Fear: A History of Images*. Cambridge: Harvard University Press. 1988. Pp. 535. \$29.50.

This book is a complex, fascinating history of nuclear culture and the connections between culture and policy on nuclear weapons and power. Emphasizing but reaching beyond American culture, and ranging over but beyond the twentieth century, Spencer R. Weart's work complements, in particular, Paul Boyer's closer study, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (1985). It also corrects one weakness in Boyer's treatment, which tended to see the bomb's advent as shaping culture rather than as given meaning by it. As Weart puts it, simply enough, "The public . . . could only understand the news [of Hiroshima] in terms of what

they already had in their heads" (p. 103). Weart shows convincingly that the images and notions characterizing the postwar "nuclear fear" were widespread, if still scattered and marginal, before Hiroshima. The news supplied less a compelling set of facts than a nearly blank screen onto which longstanding hopes and fears were projected. Moreover, understanding the physics, politics, and diplomacy as well as the culture of nuclear energy, the author shifts comfortably among the several layers of his story and weaves them together well. He is especially effective at unraveling something already known but poorly understood: how the images and emotions built up around nuclear weapons and power reflected and intensified larger divisions within modern culture over technology and authority.

Ambitious books are vulnerable to quibbles and to differing tastes in scholarship. Some readers will regard as heavy-handed or suspect the use of a variety of disciplinary methodologies and perspectives, although their use also testifies to Weart's range as a scholar. Others may tire at the sheer detail and at the story's tendency to spill out in so many directions, although at critical points Weart brings the story back into focus. Sticklers for accuracy will notice how Weart is so eager to prove that "nuclear weapons might be useless in every way" (p. 139) that he ignores their widely argued utility to Dwight Eisenhower in ending the Korean War in 1953.

More important, some simplistic distinctions, spelled out most baldly in a closing personal note, also run through the book. One is embedded in his plea for "a moratorium on the archaic images that incite such emotions" (pp. 430–31) about nuclear energy—as if images derive their power from a literal accuracy rather than from evocation and exaggeration of deeper hopes and fears. A concluding request to imagine the future in Manichean terms—a time when "every town in the world might look like central Hiroshima in August 1945" or "like the finest quarters of Paris or better" (p. 431)—restates rather than reflects critically on the pattern of posing stark alternatives that has defined so much reaction to nuclear energy.

But this book's ambition and many merits easily overshadow such quibbles. Only the absence of visual illustrations—curious in a book explicitly about images—limits the weighty supporting apparatus for this book, whose extensive notes comprise a fine guide to the burgeoning literature on nuclear weapons and power. Above all, Weart generally avoids the polemical or apocalyptic stances easily adopted in such a history and the scholarly nitpicking that is just as tempting. In-

stead, he advances large interpretations, offered firmly but unobtrusively and often elegantly.

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DAVID BURNETT KING. *The Crisis of Our Time: Reflections on the Course of Western Civilization, Past, Present, and Future*. Cranbury, N.J.: Susquehanna University Press or Associated University Presses, Cranbury, N.J. 1988. Pp. 265. \$33.50.

Near the end of his book, David Burnett King repeats the advice of Oswald Spengler, "who probably deserves to be quoted at least once in a work of this sort. Spengler said that we must 'do the inevitable or nothing at all'" (p. 225).

King has written an eminently Spenglerian book, hammered out in the somewhat mechanical fashion of the well-made college lecture course, dry, clear, and concise—as Spengler surely was not—yet full of the German historian's insistent fatalism. In King's version of history, the theme is not the relentless decline of the West but the relentless decline of modernity.

We find ourselves, writes King, in the throes of the fourth great transition in Western history. As the civilization of Greece and Rome supplanted the prehistoric West, as medieval civilization supplanted the classical epoch, and modernity the Middle Ages, so now "the modern impulse" has exhausted itself and a more austere, neomedieval "postmodern" era impends.

Most of King's book consists of catch-all generalizations about the contours of Western history since the Greeks. Each age is inventoried under four headings: style (art, music, literature), thought, politics, and economics. In matters aesthetic, for example, Western culture has oscillated from abstract (prehistoric) to naturalistic (classical) to abstract (medieval) to naturalistic (modern); predictably, the nascent postmodern culture will be abstract again. Such great changes do not occur, however, in isolation from one another. For King, civilizations are all of a piece. When decay sets in, although it may appear first in one department of life and then spread to others, it pervades the whole structure, and the collapse is universal.

Addressing "the crisis of our time," King argues that in some respects the modern age has already faded. The confident, rational, humanistic culture of modernity began to disintegrate as early as the 1870s. The "modernist" art, literature, and music of the twentieth century is actually postmodern. In thought, new antimodern religious and transcendental currents challenge the orthodoxies of Renaissance and Enlightenment. In politics and eco-

nomics, as the quintessentially modern institutions of the nation-state and the capitalist open market gradually erode, a new order takes shape on the far horizon. Individualism yields to communalism, liberty to authority, policies of limitless growth to policies of closely regulated stability.

King lays no claim to omniscience. He takes pains to qualify every generalization and hedge every forecast. He is also careful to dissociate his own values from those of the approaching post-modern world. Nevertheless, the generally Spenglerian tenor of his text is unmistakable. For King, history is a juggernaut beyond our power to resist.

Many readers will protest or scoff. Some will be badly confused by the author's use of the term "postmodern"; he shows little awareness of the quite different meaning attached to it by an army of contemporary critics in the humanities. Other readers will recoil from his professorial love of numbered lists and tidy recapitulations.

But this remains an important book, one of the few written by a historian in recent years that surveys the whole sweep of Western history and extrapolates a credible (if distasteful) future. Unfailingly lucid and logical, it directs our attention to the largest issues a scholar can raise. No one who gives it a serious reading is likely to forget the experience.

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Geoffrey Parker. *The Military Revolution: Military Innovation and the Rise of the West, 1500–1800*. (The Lees Knowles Lectures, 1984.) New York: Cambridge University Press. 1988. Pp. xvii, 234. \$29.95.

Nearly thirty-five years ago, Michael Roberts suggested that between 1560 and 1660 European warfare changed to such an extent that its various interlocking developments amounted to a "military revolution." The four major strands of this gradual, but in the end overwhelming, upheaval were tactical change, as sword, lance, and pike were replaced by missile weapons—the arrow and the musket; growth in the size of armed forces; the development of continuous and long-ranging strategies, made possible by the new armies; and the impact of the expansion of war on society. By imposing a certain order on a great variety of events, the military revolution proved a useful concept. It retains its value as long as it is not rigidly applied, as long as the dates Roberts proposed are taken as approximations, and as long as it is understood that the revolution was brought

about by forces beyond military technology and warfare as much as by strictly military causes. Above all, we should not try to interpret everything about war by the concept but use it in conjunction with other factors, particularly the growth of the modern centralized state.

Geoffrey Parker discusses the military revolution from a worldwide rather than a European perspective. He specifically excludes from his analysis such issues as the impact of war on society or the reciprocal relations between a state and its military system, although inevitably these topics cannot be kept out altogether. Instead he asks what elements in the military institutions and practices of early modern Europe enabled the West—"initially so small and so deficient in most natural resources" (p. 4)—to defeat larger native forces in Africa, the Americas, and Asia. The first chapter outlines the innovations of the military revolution, emphasizing developments in fortification and artillery. The second—a particularly strong discussion—is devoted to issues of raising and supplying the new armies. The third deals with naval warfare. The fourth chapter analyzes the confrontation of European traders and colonists with overseas societies and their armed forces. The author agrees with the thesis that superior arms were only one factor in the victories the Europeans usually gained. Of great importance as well were differences in social organization and the structure of military institutions, the difficulties other societies experienced in adapting European methods to their own ways of fighting, and different cultural attitudes toward war. A society that usually waged war to acquire slaves and fashioned its methods accordingly, to mention only one example, would be handicapped when facing Europeans intent on killing as many of their opponents as possible.

Parker tells his story clearly and with a wealth of detail, statistics, and technical information. Some major aspects of the military revolution—for instance, achieving the effective interaction of infantry, cavalry, and artillery—are discussed by example rather than developed systematically. The comparisons between European and non-European practices could have been strengthened by analyzing such Western innovations as the expansion and greater articulation of command structures. But that would have returned us to the excluded area of the interaction of society, the state, and military institutions. The material on weapons, tactics, and operations that Parker does discuss is rich and fascinating. He has written a wide-ranging survey of an important phenomenon in world history, which should prove of inter-

est to many more readers than specialists in the history of war.

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HUGH TINKER. *Men Who Overturned Empires: Fighters, Dreamers, and Schemers*. Madison: University of Wisconsin Press. 1987. Pp. xv, 272.

This book originated as a series of public lectures that Hugh Tinker gave at the University of Wisconsin, Madison. It contains two introductory chapters, watch-pocket biographies of eight men—Mohammad Ali Jinnah, Jawaharlal Nehru, Aung San and Thakin (U) Nu, Sukarno, Ho Chi Minh, Kwame Nkrumah, and Jomo Kenyatta—and a brief conclusion. The reader new to the subject of the post-World War II collapse of empires will learn much from Tinker's descriptions of the roles these men played in the independence of Pakistan, India, Burma, Indonesia, North Vietnam, Ghana, and Kenya. Those who know something about the topic will find the book less instructive.

Tinker notes in his introduction that he hopes to go beyond the British, Asian, and African "analysts" who content themselves with "Transfer of Power" or "Freedom Struggle" models (p. xii). The implication is that he will provide an alternative view, but he does not. In addition, his choice of leaders is questionable. If anyone "overturned" the British empire in India, it was Mahatma Gandhi, not Nehru. Nkrumah is a defensible choice in Africa, but, compared to Julius Nyerere, Kenyatta is not. In his conclusion Tinker writes, "The reader should have been convinced that these eight men did indeed become the instruments of their countries' re-emergence into the international community . . . and re-established respect and dignity for their peoples" (p. 238). Few, if any, of the countries Tinker discusses here had ever been enough in the international community to reemerge, and it is difficult to believe that the international community had ever given them much respect and dignity. More to the point, the two criteria fit Nyerere far better than Kenyatta. Rather than overturning the old order, Kenyatta perpetuated it. Nyerere established an entirely new one.

The strongest parts of the book are those that deal with Burma, India, and Pakistan. Tinker lived and worked in prepartition India and has written about all three countries. He admits, however, that "inclusion of the two African leaders has stretched my scholarly credibility to the limit" (p. xiii).

His discussion of Ghana seems error free—apart from his description of Dennis Austin and John Fage as American scholars (p. xv). But the Kenyatta chapter contains many factual errors and misspellings of names. Lord Delamere's farm at Njoro was not "in the heart of the Kikuyu country" (p. 208), for example; nor was General Edward Northey the "first governor" (p. 213). And it is Joseph Kan'gethe, not Kangetha (p. 218), and Peter Mbiu, not Mbiya (p. 222), and so on.

Perhaps such criticism is just academic carping. Tinker aimed his book at the general reader, not at the African or Asian specialist. And maybe because he is writing about people and events that helped define his youth, his portraits occasionally have a soft-focus quality that turns warts into beauty spots. But the various transitions from colony to independent state in Africa and Asia still affect us, and more people should know more about them.

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SHMUEL ALMOG. *Zionism and History: The Rise of a New Jewish Consciousness*. Translated by INA FRIEDMAN. New York: St. Martin's or Mages, Jerusalem. 1987. Pp. 330. \$29.95.

Shmuel Almog's thoughtful, well-researched study treats Zionism not as a social and political movement responding to the immediate pressures of the day but as a call for a fundamental change in the Jewish situation that involved an intricate mix of attitudes toward the Jewish past. Modern nationalism has been a Procrustean bed, forcing widely differing traditions and societies into a common mold. Seemingly a break with earlier nineteenth-century aspirations, Zionism entailed confronting and reshaping historic elements of the Jewish tradition according to one of several competing prototypes of Jewish nationhood.

Almog's study refrains from narrating once again the early history of the Zionist movement, its leading personalities, and its factions. Instead, he meticulously disassembles the tangled skein of disputes that preoccupied Zionist speechmakers and writers in the formative decade between the publication of Theodor Herzl's *Judenstaat* in 1896 and the Helsingfors Conference of Russian Zionists in 1906.

Zionist ideas were enunciated well before 1881, of course, but the movement became, during the period that Almog's book covers, a diaspora-wide phenomenon and produced a considerable and articulated periodical literature, some of it now forgotten. There seemed at the time to be two principal ideological poles—Herzl and his associ-

ates emphasizing political activism, visionary daring, and a transformed status of the Jews in an era of growing anti-Semitism and Ahad Ha-Am (Asher Ginzberg) and his group of "spiritual Zionists" who gave priority to cultural values, the morale of the people, and the "question of Judaism" in an age of positivism and science. Historical reality was more complex, and Almog draws on a large number of figures (some well-known members of the new Jewish leadership class that Zionism constituted, some individuals who flirted only briefly with Zionism) to depict the overlapping series of issues that Zionism was forced to confront.

The first and most abstract layer of issues dealt with the nature of Jewish nationhood and the preoccupation with rehabilitating Jewish dignity: these reflected various theoretical and ideological disputes on Jewish national unity or lack of it in relation to late nineteenth-century conceptions of race, notions of historical determinism and voluntarism, and the tradition of Jewish messianism. A second level revolved around the call for Zionist "cultural work" in the diaspora, which provoked heated discussions of the role of religion and the participation of the Orthodox in an essentially secular Jewish movement. This complex of concerns raised the question of the relation of the nascent Zionist culture to European civilization and the expiring Jewish Enlightenment and the relevance of a Nietzschean transvaluation of values to a Judaism supposedly warped by centuries of diaspora existence. A third level surfaced with the failure of Herzl's diplomatic activities, and some took as their slogan the idea of *Gegenwartsarbeit*, concentration of effort on practical work both in Palestine and in the diaspora. In Eastern Europe, Zionism found itself in competition with non-Zionist or anti-Zionist secular Jewish movements at a time when the revolutionary mood seemed to indicate that sweeping changes were about to occur in the Jewish situation, forcing a Zionist confrontation with various conceptions of communal self-government and diaspora autonomy.

Almog's presentation concludes with a fourth level, the struggle that erupted over the possibility that an area of British East Africa might be available for Zionist colonization—the so-called Uganda dispute, which precipitated the formation of a separate "territorialist" organization seeking a Jewish homeland somewhere besides Palestine. Almog implies that the conflicts with diaspora autonomism and territorialism not only confirmed the centrality of a "historic attachment" to the land of Israel but also clarified pragmatically the pluralistic nature of Zionism at the heart of the

movement's widespread appeal and continued flexibility.

Although Almog has an inordinate fondness for using the language of paradoxes, polarities, and dialectic relationships to describe the complex weave of issues and positions he summarizes, his lucid comparisons and contrasts, and the useful bibliography, make this book a most valuable contribution to the literature on early Zionism and its pivotal place in modern Jewish intellectual history.

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FATMA MÜGE GÖÇEK. *East Encounters West: France and the Ottoman Empire in the Eighteenth Century*. (Studies in Middle Eastern History.) New York: Oxford University Press. 1987. Pp. x, 192. \$27.50.

This short but extremely insightful and useful study is a welcome first product of the efforts of the Institute of Turkish Studies (Washington, D.C.) to promote and encourage the development of Turkish and Ottoman studies throughout the world. Fatma Müge Göçek examines the impact of the embassy to Paris of Yirmisekiz Çelebi Mehmed Efendi in the later years of the reign of Ahmed III (1703–30), the time of the so-called Tulip period, when the Ottoman empire first began to break down the long-standing "iron curtain" that had separated it from Europe during the centuries of Ottoman power and greatness.

Although Göçek's detailed account of Mehmed Efendi's travels and experiences, and in particular of his perceptions and understanding of what he saw, should be of great interest not only to Ottoman but also to European historians, the real value of her work lies in her account of the cultural and social effects in Istanbul of the embassy itself, based largely on Mehmed Efendi's long-neglected travel account, *Sefaretnâme*, sent to the sultan and his modernizing grand vizier, Ibrahim Paşa. Göçek describes the basic differences between the Ottoman and French societies as they existed at the time. Among other things, she points out the substantially different boundaries that existed between the public and private spheres of life, the different sorts of music, different roles for women, different manners and customs, different concepts of the role of government, different types of entertainment, literature, art, architecture, and the like. She concludes that the mission, and in particular Mehmed Efendi's reports, had important effects on Ottoman society, particularly in changed habits of dress and food

consumption, construction of Western-style palaces, knowledge of foreign languages, development of an Ottoman printing press, and communication with foreigners in Istanbul—all of which characterized the Tulip period.

One is left wondering, however, the degree to which the impact of those reports, and the activities of Mehmed Efendi himself, extended beyond the very limited circle of ruling-class members who were aware of them, and one remains uncertain whether those reports were in any way connected with the far more significant military, social, and economic reforms that came to the empire starting a half century later, particularly during the nineteenth-century *Tanzimat* reform period. Those changes, often in direct emulation of the West, reflected little regard for the Middle Eastern heritage and in many cases contributed to the grave problems that disrupted and ultimately destroyed the empire in the early years of the twentieth century.

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VICENTE L. RAFAEL. *Contracting Colonialism: Translation and Christian Conversion in Tagalog Society under Early Spanish Rule*. Ithaca: Cornell University Press. 1988. Pp. xiii, 230. \$28.95.

Although Philippine historians have studied the conversion of lowland Filipinos to Christianity, little attention has been paid to the dynamics and the processes of the conversion. In a pioneering work, Vicente L. Rafael approaches this topic by examining the history of the communication between the Spanish and the Tagalogs. Rafael focuses on exploring the relationship between translation and conversion and the role each played in the development of Tagalog colonial society between the late sixteenth and the early eighteenth centuries.

Rafael begins by discussing key differences between Spanish and Tagalog views of translation. To the Spanish the translation of Castilian into the Tagalog vernacular was essential to evangelization. Translation involved a ritualistic acknowledgment of one's indebtedness to God and also permitted the Spanish to introduce their ideas of authority into Tagalog society. Those purposes are reflected in the publication by Spanish missionaries of Tagalog *artes* (grammars) and *vocabularios* (dictionaries). But Tagalogs viewed translation quite differently. After a close analysis of an early seventeenth-century Tagalog work designed to teach Tagalogs Castilian, Rafael concludes that Tagalogs valued Castilian not as a tool to commu-

nicate but as a means of deflecting the shock of Spanish rule. Exposure to Castilian would prevent the Tagalogs from being overwhelmed by Spanish demands and enable them to incorporate elements of Spanish culture.

The remainder of Rafael's work provides a detailed examination of the interrelationship between conversion and translation. The translation of Christian doctrines from Spanish into the vernacular enabled the Tagalogs to respond to the doctrines in their own way. Rafael asserts that Tagalog conversion was based on conventions that differed from those of Spanish missionaries and that their translation of Christian doctrines reflected Tagalog ideas of exchange and hierarchy. He explores the role of the values of *hiya* (shame) and *utang na loob* (debt of gratitude) in this process. For instance, Rafael contends that the Tagalog fascination with confession represented a means of avoiding *hiya* and establishing ties of *utang na loob*. He also states that the presence of untranslatable Castilian religious terms in Tagalog increased the ability of the Tagalogs to respond to Catholicism in their own manner by permitting them to imagine a range of possible associations not originally intended by the Spanish missionaries.

By examining the social and political effects of conversion, Rafael also explores the process by which conversion was translated into submission to Spanish rule. In order to maintain conversion the Spanish sought to recast Tagalog society. For instance, the colonial regime gained the support of the Tagalog elite by granting them certain rights and privileges, thus altering the nature of indebtedness and servitude that had characterized preconquest Tagalog society. According to Rafael, the submission of other Tagalogs resulted from the "reinvention of death" that accompanied conversion. Vernacular poems and Tagalog translations of Spanish discourses on death created an attractive image of death that appealed to many Tagalogs. Rafael concludes that during the early Spanish period translation enabled the Tagalogs to negotiate with Spanish authorities and to evade Spanish demands. Translation thus permitted the Tagalogs to distinguish between Spanish and Tagalog interests. Moreover, the variety of Tagalog translations indicates that many types of conversions were possible.

Rafael provides an innovative and a fascinating interpretation of the early Spanish period. His conclusions are based on a masterful use of primary sources. He draws on Spanish grammars, dictionaries, religious manuals, and early accounts of Tagalog society. Even more important is Rafael's use of Tagalog sources such as poems, confessional manuals, and prayers to the dying,

sources largely ignored in earlier treatments of this period. Rafael's careful analysis of those sources permits him to provide a more detailed examination of the Tagalog response to the Spanish than has been offered in other studies. Tagalog scholars will want to check the accuracy of Rafael's translations and to evaluate his arguments based on linguistics. Even though some of his conclusions may prove controversial, Rafael has written a challenging and insightful work. His focus on translation and conversion makes his book of great value not only to Philippine historians but also to scholars interested in the role of language in history. This study is a work that deserves to be read and reread carefully.

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UDO RATENHOF. *Die Chinapolitik des Deutschen Reiches 1871 bis 1945: Wirtschaft—Rüstung—Militär*. (Militärgeschichtliche Studien, number 34.) Boppard am Rhein: Harald Boldt. 1987. Pp. ix, 629. DM 96.

Udo Ratenhof's monumental treatise stands among the most ambitious books on the history of Sino-German relations to date. Based on an exhaustive study of the literature and of nearly all archival material accessible to Western scholars, this study rectifies the weaknesses of earlier monographs that treat the links between German and Chinese history more as exploratory contacts between alien empires on opposite sides of the globe. Ratenhof presents Germany's and China's historical development since the 1860s in tight paragraphs but in plenitude. He also gives us "the policies of the other great powers in order to outline the basic pattern of international relations and to show . . . what freedom of movement remained to Germany in the Far East" (p. 14). The enormous scope of this undertaking explains Ratenhof's decision to limit his study to the behavior of the German military-industrial complex and the calculations of the Wilhelmstrasse. We find scarce mention in this book of German Sinology or the fascination of Chinese intellectuals with German thought.

The author is forced to retrace his steps frequently in order to bring parallel developments in China, in Germany, and elsewhere in the world up to date. The consequent strain on the reader might have been avoided by the use of subheadings to set off excursions into the history of, say, the Krupp concern or British-French colonial rivalry before 1904. The summary paragraphs scattered throughout the book are useful, although they also indicate that, ultimately, Ratenhof does

not require us to alter the established views on Germany's interests in the Far East and on the Chinese revolution. One exception is his repeated references to alleged plans in the 1890s for a German-Russian-Chinese bloc to dominate Europe and Asia. It would have been particularly interesting had he dwelt on those plans more extensively and cited relevant documentary proof in full text.

The author scrupulously observes scholarly neutrality as he moves through a period filled with passionate conflicts. At the same time, can one really appreciate the Boxer War without some knowledge of the barbarity perpetrated by both sides? Can one summarize in two lines the intellectual and psychological shock of Chinese intellectuals at life in modern Germany (p. 175)? Ideological questions are almost completely avoided here. This is not meant as criticism. There was so little common ideological ground between the two countries. Ratenhof does bring up Kang Yü-wei's interest in the cyclical theory of history and its bearing on the concept of universal progress, but the point is not discussed. He gingerly points to "militarism" in Prussia and modern China but offers no comparison of Junker militarism (which was integral to the culture of Prussia and had an ideology) and Chinese militarism (which was alien to China's tradition and an outgrowth of rampant social and moral decay). Alexander von Falkenhäusen, for one, clearly saw the different role of the soldier in Europe and China in the twentieth century. But then personalities are not at the center of this study. There is no mention of Lenin and Stalin, no portrait of Chiang Kai-shek, Martin Fischer, or Oskar Trautmann. The head of the Chinese trade department, Yü Ta-wei, is described only as "arrogant," ignoring Yü's distinguished scholarly standing that put him intellectually head and shoulders above the Chinese community in Berlin. This is regrettable because there were many Germans and Chinese for whom contact with a very different civilization was a powerful personal experience—among the Chinese, in particular, because, as Ratenhof so rightly concludes, they were the ones who had the most to learn.

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OSCAR J. MARTINEZ. *Troublesome Border*. (Profmemex Monograph Series.) Tucson: University of Arizona Press. 1988. Pp. xii, 177. \$22.95.

In this study, borderlands specialist Oscar J. Martínez offers his perspectives on the Mexican-U.S. frontier. Using conflict as his unifying theme,

Martínez discusses the continuing struggles to establish the boundary and then analyzes the effects of these disputes on Indians, Chicanos, and Mexican citizens living in the region. Finally, he surveys recent friction stemming from controversies over the environment, economic development, immigration, and cross-border travel. Not surprisingly, he concludes that both nations must display a greater willingness to cooperate if the intense nationalism, racism, and mutual suspicion bred from one hundred fifty years of conflict are to be overcome.

As a synthesis, the book presents little new evidence. Still, its brief analysis of contemporary issues is useful, as is the author's sympathetic discussion of the dilemmas faced by Chicanos of the borderlands, torn between the desire to maintain their traditions and the demands of their new country north of the Rio Grande. Martínez also offers a fine description of northern Mexico's physical and psychological separation from the rest of the nation. The international border and the region's resulting ties with the United States exacerbate tensions between *norteros* and Mexico City, adding to the unique struggles of borderlands residents. The fear of the frontier zone, Martínez points out, is not limited to Washington, as Mexico City often views the border regions with suspicion and apprehension.

Martínez occasionally compares conflicts along the Mexican-U.S. border with those on other international frontiers. As he explains, it is helpful to recognize that border zones, by nature, tend to be lawless and prone to revolution. From this broader context, he concludes that the region under study is neither the most peaceful nor the most troubled of the world's international frontiers. Admittedly speculative, such musings should provoke vigorous comparative analysis.

Unfortunately, this study is not without flaws. Numerous errors mar the first two chapters. Readers will be surprised to find that Martin Van Buren was president of the United States in 1829 (p. 12), that James W. Polk oversaw the territorial expansion of the 1840s (p. 37), and that Howard Lamar served as president of the Republic of Texas (p. 36). In addition, the author's analysis of U.S. actions would be more convincing and less simplistic if standard secondary works, such as Francis Paul Prucha's numerous books on Indian policy, had been consulted. Moreover, discussions of nineteenth-century conflicts over the boundary line, of filibustering, and of Indians are often incomplete and misleading. Martínez fails to capture the true complexity of the international rivalry. He surveys the conflicts of the early 1840s, for example, without discussing Mexico's two incursions against San Antonio or the Texas coun-

terthrust against Mier, events critical to understanding the bitterness that characterized the era.

In conclusion, the present volume should be used with care. Engagingly written and logically organized, it offers readers the conclusions of a recognized borderlands scholar. In his brief synthesis, Martínez avoids the narrow parochialism and stilted style of so many scholarly works. Better editing, however, and a more balanced presentation of nineteenth-century tensions on both sides of the border would have improved the final product.

ROBERT WOOSTER
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NORBERT MACDONALD. *Distant Neighbors: A Comparative History of Seattle and Vancouver*. Lincoln: University of Nebraska Press. 1987. Pp. xxii, 259. \$27.95.

The title of Norbert MacDonald's new book aptly states the main focus of his comparative historical study of two Pacific Coast American and Canadian metropolises. Seattle and Vancouver, both located on Puget Sound, are fewer than one hundred miles apart. Until the middle of the twentieth century, they experienced a remarkably parallel development. Both started out in the mid-nineteenth century as remote lumber ports, with recognized potential because of their excellent harbors. In the 1880s cycles of boom and bust followed the arrival of transcontinental railroads. Although the Klondike gold rush of the 1890s helped Seattle more than it did Vancouver, the fortunes of both places depended on the growth of the Canadian West and the American Northwest. The early twentieth century saw the rise of agribusiness. World War I had little impact; the twenties ushered in a wave of prosperity; the thirties brought depression; and World War II saw a tremendous upswing. In the forty years after hostilities, the cities followed increasingly divergent paths, wedded to national policies framed in Ottawa and Washington, D.C. By the 1980s, both Seattle and Vancouver had metropolitan populations of more than one million.

More than ever before, the cities were distant neighbors, separated by the Forty-ninth parallel, located in different nations with distinct governments and traditions. After all, Canada, unlike the United States, is a constitutional monarchy with a large French minority. Canadians have long accepted, as witness hundreds of crown corporations, that the government of Canada should play a significant role as a partner with private enterprise in promoting national development. Indeed, Ottawa's commitment to a transcontinental rail-

road helped persuade British Columbia in 1871 to join the Confederation. MacDonald explains, "Differences in the actual federal systems of the United States and Canada contribute to the uniqueness of Seattle and Vancouver. Political centralization has proceeded much further in the American setting, and average Seattleites are more directly influenced by the activities and programs of their federal government than their counterparts in Vancouver. . . . Through judicial interpretation and the long-term provincial-federal conflict, Canadian provinces have increased their power and authority, while the federal presence in cities has remained more limited and fragmented" (pp. 200–01).

MacDonald's study is traditional and straightforward. His narrative takes into account the character of the dominant groups, the brutal treatment of Asians, the frequency of labor disorders, the tedious nature of the local political scene, the attempts at planning, the role of the middle class, and the rise of the military-industrial complex. These and other matters are documented through the extensive use of contemporary newspapers, census records, city records, and secondary materials.

MacDonald has, he says, deliberately sought to avoid a narrow "somewhat artificial slice of the city's total experience," noting: "I had read enough of the new urban history to know that although such techniques often provided valuable new insights, they did not automatically guarantee significant findings. Indeed in many cases such elaborate analysis did little more than restate long-accepted historical themes" (p. xvi). The result of self-imposed restraint is a cautious book that does not go very far beyond the basic well-known story of the two cities. MacDonald concludes with a hardly startling observation: "All one can predict with any confidence is that as in the past, each city will follow its own separate path" (p. 204). Because difficult questions are avoided, from those concerning the search for a Canadian national identity in Vancouver to the lack of a boss system in Seattle, this book offers little that is new beyond an innovative comparative approach.

LAWRENCE H. LARSEN
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RAYMOND A. ESTHUS. *Double Eagle and Rising Sun: The Russians and Japanese at Portsmouth in 1905*. Durham, N.C.: Duke University Press. 1988. Pp. x, 265. \$35.00.

The subtitle of this book describes adequately the emphasis Raymond A. Esthus has given to his

study. He is concerned more with the convocation, conduct, and documentary outcome of the conference than with the Russo-Japanese conflict and its causes. The book is comparable in this respect to Tyler Dennett's *Roosevelt and the Russo-Japanese War* (1928) and Eugene Trani's *The Treaty of Portsmouth: An Adventure in American Diplomacy* (1969). It is, however, more inclusive than Dennett's book and is similar to Trani's work; like Trani, Esthus treats the subject as "an adventure in American diplomacy" rather than as an episode in Russo-Japanese relations. Esthus's book is, nevertheless, of better quality than Trani's.

There are four principal components of this work. The first of these is introductory; the first two chapters deal respectively with Russia and Japan. The contents of these chapters are concerned to some extent with the issues that gave rise to the war. But Esthus devotes considerable attention to various matters other than the inception of the war. A second component comprises the third chapter, which concentrates almost entirely on the diplomatic preliminaries of the conference. The author does touch on more fundamental matters, such as the role of President Theodore Roosevelt in the negotiations preceding the conference and the capacity of the two combatants to pursue the war further. The third part of the book, consisting of chapters 4 through 16, is the main body of the study. Chapter 17 constitutes the fourth part of the book and is to some extent the best. It deals with the aftermath of the conference and, although only a brief summary, it is concerned with the issues apart from the conference.

One of the consequences of treating the subject as "an adventure in American diplomacy" is that the settlement of the war appears to be only in part related to the war itself and its causes. An example of this focus is the treatment of the very basic question of Japan's capacity to endure a more protracted war. The author notes that the Japanese commander in Manchuria, Field Marshal Oyama Iwao, and his chief of staff, General Kodama Gentaro, informed the government in Tokyo in March 1905, after the battle of Mukden, that it would be impossible for Japan to carry on the war. The message they sent was, however, somewhat different from Esthus's description in both tone and significance. It conveyed the idea that the army would be unable to carry on without a further commitment of troops and financial support. It added that the Japanese had underestimated the capacity of the Russians to commit further troops of good quality to the Manchurian battle front, to transport forces and equipment there, and to match Japan in financing a continuation of the war. This version clearly left the Japanese government with a clear choice as to

whether it would have to accept the verdict as it evolved in the conference or prepare to regroup and carry on.

On balance this study has, in spite of some considerable drawbacks, a number of merits. It is readable from the standpoint of both style and interpretive content. It includes interesting and informative detail of events and personalities. It is a useful sketch of the settlement and includes helpful footnotes, a bibliography, and an index. There remains, however, a marked distinction between the substance of the negotiations and the events that gave rise to them. This study has the effect of isolating to a great extent the substance of the negotiations from the reality of the conflict.

JOHN ALBERT WHITE
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BENJAMIN D. RHODES. *The Anglo-American Winter War with Russia, 1918–1919: A Diplomatic and Military Tragicomedy*. (Contributions in Military Studies, number 71.) New York: Greenwood. 1988. Pp. xii, 156. \$35.00.

British and American intervention in the Russian civil war must be one of the most overwritten topics in history. It is easy to understand why Soviet historians have been so very interested. They have always pretended that the Red Army defeated not only the domestic counterrevolution but also the forces of world imperialism. And, when nationalism became a cornerstone of Soviet politics, the publicists could maintain that the Bolsheviks had fought for the nation in 1917–21. It is harder to fathom what draws Westerners to this topic. It is, after all, difficult to argue that the few thousand British and American soldiers in Murmansk and Archangel made a difference in the course of the Russian civil war. In the course of the entire northern intervention, 222 Americans died (Benjamin D. Rhodes's figure). And it is naive to believe that the Bolsheviks' hostility to the West was in any way caused by what the "imperialists" did or did not do. Presumably Westerners like to show the stupidity or wickedness of their own governments. Or maybe they are attracted by the exotic locale.

Rhodes has nothing significant to add to the picture that we already have. We already knew what is worth knowing from the excellent books of George Kennan and Richard Ullman. Indeed, the entire venture was ill conceived; the Allied generals knew little about the complex political world in which they found themselves; the soldiers had no idea what they were fighting for; and the Americans and British suffered because of the primitivity of the area, the bad food, and the cold.

Rhodes is not posing questions in which genuine historians are interested. He does not venture a novel interpretation or bring to light facts that would enable others to do so. He knows no Russian and is as knowledgeable about the course and issues of the Russian civil war as the average American soldier was in 1918. His slender book is filled with trivia.

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ANCIENT

ANTHONY M. SNODGRASS. *An Archaeology of Greece: The Present State and Future Scope of a Discipline*. (Sather Classical Lectures, number 53.) Berkeley and Los Angeles: University of California Press. 1987. Pp. xv, 218.

In this small, attractive book with the cumbersome title, Anthony M. Snodgrass presents us with something that is not so much an archaeology of Greece as a bright illumination of several sides of the archaeology of Greece (and of the rest of this discipline as well). The six lectures printed here clearly show the range and depth of the author's scholarship as well as his gift for subtle instruction of his readers, whether they be professionals or not. Lacking the orderly coherence of a monograph, as such collections tend to do, the book nonetheless shines with unusual information and fresh insight.

Its horizons are wide. The first chapter, in a courteous style rare for an address in an already tediously old debate, examines the state of classical archaeology in the context of the prescriptions of the "new archaeology" and reflects in passing on the neglect and disdain shown by the new for the old. The disdain appears to have left scars; the subsequent and not entirely persuasive enumeration of the merits of classical archaeology has a curious undertone of apology entirely unwarranted in view of the fascinating tapestry it weaves.

Fascinating documentation is, in fact, a special strength of this book. That aspect is very evident in the second chapter on the differences in goals, methodology, and style between ancient history and classical archaeology. The examples chosen to illustrate each of the many points in this discourse deserve close attention from historians and archaeologists. Next come two chapters dealing with the rural landscape of ancient Greece. The first views this topic from the perspective of ancient authors who displayed an almost universal disregard for what lay between town and town, but a few interesting and not widely known exceptions are cited. The companion chapter discusses survey

archaeology as a means to elucidate ancient geography. It is evident that it was written several years ago, and there has been much progress on this subject. Worse is that Snodgrass almost entirely ignores the key role of palaeo-environmental studies in this interdisciplinary approach. The subject of the chapter, a survey of central Boeotia, merits suspicion because Snodgrass failed to detect significant Mycenaean rural settlement within sight of the walls of Thebes.

The fifth chapter reorients the reader toward a contemplation of the meaning of figure scenes in early Greek art. This is a subject well outside my competence; possibly for that reason I found this chapter one of the most interesting parts of the book, thought-provoking in its treatment and elegant in style. Alas, the abominable quality of all photographic reproductions in the book is particularly unpleasant here.

Entering the final chapter deceptively called "The Early Iron Age in Greece," readers will discover that, on the home ground of the author, they have been subtly lured to consider the fundamental processes of scholarly research, from the gathering of data and the first assessment by way of the formulation of alternative working hypotheses to that vague land beyond in which those hypotheses must somehow be tested, rejected, or validated. Existing data have long suggested a strong cultural regression at the end of the Mycenaean period accompanied by a striking dearth of archaeological remains. In a fine clear essay, these findings are examined, and their traditional explanations are found wanting. Snodgrass then presents his own hypothesis of greatly enhanced reliance on pastoralism, finds it wanting in support, too, but perhaps holding promise in the future. In this chapter, I think, we recognize the main merit of the book, which is not as a treatise on Greek archaeology but as a discourse on the method of scholarly inquiry, an effective one because it will lead readers to acquire their own insights rather than to rely on the *ex cathedra* statements of principle by such scholars as David Clarke and Lewis Binford dutifully cited by the author.

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ROBERT GARLAND. *The Piraeus from the Fifth to the First Century B.C.* Ithaca: Cornell University Press. 1987. Pp. viii, 280. \$44.50.

During Athens's heyday, the fifth and fourth centuries B.C., the Piraeus was not only the commercial center of the Eastern Mediterranean but also a mighty naval base, because it harbored the Athenian fleet. Indeed, to serve as such was its

original reason for being, along with forming an essential element in Athens's defensive arrangements. A by-product of the city's role as naval base was a profound effect on political development in Athens. The oarsmen of the war galleys were mostly thetes, the lowest class in the Athenian citizen body; rubbing shoulders daily in the streets and squares of the Piraeus, they gradually coalesced into a group of like-minded voters, the hard core of Athens's radical democrats.

By Pericles' time, very likely even earlier, the Piraeus had become a thriving commercial port as well as naval base. It flourished in both capacities until the battle of Amorgos in 322 B.C. wrote finis to Athens's fleet and Macedonian domination ended its independence.

Administratively, the Piraeus was merely a single deme but one that happened to be a major city in size. The ordinary staff of deme officials could hardly handle such a place, so Athenians helped out by assigning some of their elected commissioners to it: five of the ten *astynomoi* (city commissioners), five of the ten *agoranomoi* (market commissioners), five of the ten *metronomoi* (inspectors of weights and measures). The population of the Piraeus was as international as its trade. There were Greeks from practically all of the city-states of Greece and the Eastern Mediterranean, and there was a wide representation of non-Greeks—Lydians, Thracians, Phrygians, Egyptians, and, above all, Phoenicians. Like foreigners in all ages, these non-Greeks brought their own gods with them; at the Piraeus one could worship, alongside the standard Greek deities, Bendis of Thrace, Isis and Sarapis of Egypt, the Anatolian Mother of the Gods, the Syrian Aphrodite Ouranios and her consort Adonis, and so on.

Robert Garland presents a comprehensive account of all of this and more, with as much detail as our limited sources of information permit, supported by full documentation. He devotes a chapter to religion that is particularly good; in it he sketches the overall picture and follows with a catalogue of all known cults and foundations. A chapter on topography is less successful. Garland includes all of the major sites and monuments but makes no attempt at a structured presentation, merely offering a haphazard list. And he is somewhat unreliable on maritime matters—for example, *naukleroi* were not "shipowners or captains who transported their wares on board their own ships" (p. 68) but owners or charterers of ships that they filled with either their own or others' cargo; Roman ships, as reliefs show, docked by the prow to unload and not the stern (p. 86); the *ellimenion* was not "a passenger tax" (p. 88) but one of the harbor dues; triremes did not require "a thorough overhauling in dry docks during the six

winter months" (p. 96); they required hauling out of the water into the sheds at all times when not in use to keep from getting waterlogged; however a *penteres* may have been rowed, it was not by "two banks with three men per oar" (p. 204).

But these are minor flaws. Garland has given us a fine study, comprehensive and scholarly, of a locale that played a key role in Greek history.

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CLAUDE NICOLET. *L'inventaire du monde: Géographie et politique aux origines de l'Empire romain*. (Nouvelles études historiques.) Paris: Fayard. 1988. Pp. 345. 160 fr.

Claude Nicolet's aim is to respond to a pair of questions that have "space" (pp. 7–9) as their common denominator: why did the reign of Rome's first emperor seemingly coincide with both the fulfillment and the termination of its conquests, and was the empire by this stage a coherent entity territorially or at least developing as one? To find answers, Nicolet argues, an appreciation of Romans' geographical knowledge is required, together with a grasp of the administrative steps they took to hold conquered territory. He attaches particular significance to Augustus's *Res Gestae*. Nicolet elaborates how the work (chaps. 25–33) asserts Roman dominance of the inhabited world, in a tradition that stretched back to Republican figures (Pompey and Caesar above all), dominance that was to be confidently maintained after Augustus's death, as Nicolet illustrates from the remarkable *Sebasteion* unearthed at Aphrodisias. Roman geographical knowledge in Augustus's day is surveyed by Nicolet in chapter 3, followed by a listing of journeys of exploration during the Principate in chapter 4. An examination of Agrippa's geographical work in chapter 5 very fairly sees its purpose as both political and symbolic (p. 126). The final four chapters treat Augustus's organization and control of space with reference to censuses, land surveying, documentation (in his *Breviarium Totius Imperii* especially), and regions within Rome and Italy. A conclusion in which Nicolet might have returned to his initial questions is lacking, but there are notes on a substantial scale, a bibliography, and over fifty illustrations (which are not closely integrated with the text).

The appeal of the book lies more in its scholarship and challenging discussion than in its findings. Nicolet rightly stresses the need to view the ancient world through contemporaries' eyes (p. 14), and his arguments (pp. 88–92) that they did not lack all "map consciousness" merit careful appraisal (but compare my review of J. B. Harley

and D. Woodward, *The History of Cartography I*, *AHR* [1989]). All the same, his view of the second half of *Res Gestae* as a really precise geographical summary (pp. 20, 40, 69) seems exaggerated, as does his belief that the Roman world was well delimited as early as Augustus's day. Rather, it resembled an ever-changing patchwork and was long seen as such from both within and without (note Appian, *Preface* 7). Moreover, for all of the material gathered by Nicolet in chapter 4, there was in fact never much public or private enthusiasm for expeditions of discovery. As to maps, the impression must be that they played no part in officers' training (for military manuals, note B. Campbell, *Journal of Roman Studies* 77 [1987]: pp. 13–29); for example, the claim (p. 20) that the use of maps on campaign is implied by a passage in Velleius (2.109.3–4) does not convince.

Although Nicolet's wish to probe functions and activities of Roman administration more than personnel is admirable, in two chapters the temptation to do so from a modern viewpoint still needs to be overcome. Even if it be accepted, as the author urges in chapter 6, that from Augustus's reign censuses were taken on a wide scale (T. D. Barnes's *The New Empire of Diocletian and Constantine* [1984], chapter 14, might have been cited), it seems rash to claim that the results were wanted principally for their statistical and publicity value (p. 157; Claudius's words "publice notae" should be taken as "ascertained officially" rather than "publicized"). And the testimony of the *Tabula Heracleensis*, together with a strong personal conviction that the Roman state ought to have had an organized system for registering urban property at least as far back as the first century B.C., is no confirmation that it did (chap. 7). In both instances there is a questionable acceptance that, because Roman administrators gathered a mass of information, they shared the passion of a modern bureaucracy for retrieving and analyzing it. Though brief, N. Purcell's sober assessment of the limitations of Roman procedures can be set against this view (*Oxford History of the Classical World*, chap. 23).

Altogether it may be that Nicolet overestimates the impact made on Romans by their own documents, despite the undoubted proliferation of such material during the Principate. At the same time his book is so lively, wide-ranging, and up-to-date that it could not fail to stimulate any scholar with an interest in ancient geography or government.

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FRANK KOLB. *Diocletian und die Erste Tetrarchie: Improvisation oder Experiment in der Organisation monarchischer Herrschaft?* (Untersuchungen zur antiken Literatur und Geschichte, number 27.) New York: Walter de Gruyter. 1987. Pp. viii, 205. DM 98.

This is a useful book but a very specialized one. Because Frank Kolb concentrates exclusively on the organization of the Tetrarchy, his discussion centers on the issue of how far it represented a plan by Diocletian rather than a succession of rapid reactions to crises. The introduction summarizes the opposing viewpoints. Kolb's own convictions are decidedly in favor of planning, so that his examination of Diocletian's entry to power in chapter 2 supports the idea that Diocletian intended from the outset to share it (p. 15). Chapter 3 investigates the appointment of Maximian as successively Caesar and Augustus and the "epiphany" of the imperial *cognomina* Iovius and Herculus. It is argued (p. 49) that Diocletian envisaged a Tetrarchy even when Maximian became Caesar and that this elevation was not triggered by the Bagaudae (p. 67). Review of the appointment of Constantius and Galerius as Caesars follows in chapter 4, with predictable attention to circumstances and purpose as well as to the *dies imperii*. Consideration of the role of the divine *cognomina* (chapter 5) is followed by an analysis of the creation of a tetrarchic "symmetry" (by adjustment of *dies imperii* and regnal years) in chapter 6. Kolb's argument that Diocletian had long prepared for the abdications of the Augusti is continued in chapter 7, which Kolb splits into four sections—on Lactantius' historical reliability (urging that his factual accuracy is overrated and in any case cannot necessarily validate his views), the dynastic plans of Diocletian, Maximian's oath, and the timetable for abdication. The book concludes with a chapter on the date (ca. 300–305?) and message of Galerius' arch at Thessalonica and with an appendix on the Arcus Novus at Rome. A bibliography of main works cited, indexes, and four pages of plates (mostly coins) are also included.

Kolb's presentation is lucid and his reasoning for the most part judicious, even if it has to remain frustratingly inconclusive. Almost twenty pages of discussion (pp. 49–67) leave the date of Maximian's elevation as Augustus uncertain (July 21, 286 is rightly rejected), and the possibility that Galerius was made Caesar on May 21, 293 is reckoned to be neither demonstrable nor refutable (p. 76). Thus, since the book breaks little new ground, its value must lie rather in the painstaking appraisal of familiar issues; the thorough documentation

and the summaries of other scholars' views should prove especially helpful.

Perhaps even for specialists Kolb might have stressed more forcefully the poor prospects for determining either the framework or the details of Diocletian's policy from our pathetic ragbag of extant sources—notable not least for sloppiness (so that all of the variant testimony can never be reconciled) and a general lack of interest in the nature of the Tetrarchy. In addition, where coins and panegyric are concerned, it is impossible to know just how accurately official views are being reflected; equally indeterminable is the extent to which Diocletian took note of precedents set by earlier emperors. Moreover, with this dearth of material, unwarranted assumption can find too ready an acceptance: for example, any proof that March 1, 293 was the *dies imperii* of both Caesars cannot in itself guarantee that their elevation was planned very long in advance (p. 77).

The questions that Kolb reexamines have some importance, but, as his investigation once again demonstrates, most of them may well have to remain unanswerable. Meanwhile, potentially more rewarding and more significant aspects of Diocletian's reign await attention, and, since this book evidently developed from a projected *Forschungsbericht* on the emperor (p. 5), we may hope that Kolb will feel encouraged to turn to some of them, too.

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MARTIN GOODMAN. *The Ruling Class of Judaea: The Origins of the Jewish Revolt against Rome A.D. 66–70*. New York: Cambridge University Press. 1987. Pp. xiii, 263. \$39.50.

A number of theories have been presented to explain the causes of the Jewish revolt against the Romans in 66 A.D.: the incompetence of the Roman governors, the oppressiveness of Roman rule, offenses to Jewish religious sensibilities, the Messianic aspirations of the Jews, class tensions, and quarrels with local Gentiles. Although not dismissing these factors, Martin Goodman argues in this provocative book that the chief cause was the power struggle within the Jewish ruling class. The rulers of Judaea, he contends, clung to power by courting popularity through the advocacy of rebellion.

Goodman's theory suffers, however, from the fact that none of the three major sources for the revolt gives his reason for it. True, Josephus, a participant who defected to the Roman side, may well have doctored his interpretation of the facts.

But it is precisely because he had several predecessors whose accuracy he sharply criticizes, because he had so many personal enemies, and because he wrote his account so soon (79–81) after the war, when many of the participants were still alive and were thus able to challenge him, that he had to be particularly careful in his presentation. Moreover, Josephus is by no means uncritical of Roman rule. Whereas he commends the fairness of Julius Caesar, the emperors Augustus and Claudius, and the governors Vitellius and Petronius, he sharply censures Crassus, the emperor Caligula, the governor Sabinus, and the procurators Pilate, Cumanus, Felix, Albinus, and Festus.

True, Tacitus, who included the rebellion in his *Histories*, Book 5, did not have firsthand knowledge, wrote a full generation (104–09) after the end of the war, and was clearly prejudiced against the Jews. But it is apparent that he had imperial records and showed in his account (*Histories* 5.2–3) of the origin of the Jews that he could record some highly complimentary statements about them. True, the rabbinic account (*Gittin* 55b–57a) is brief, is given in the name of the third-century rabbi Johanan ben Nappaha, and is not recorded until the codification of the Babylonian Talmud, ca. 500, but we have every reason to believe that the rabbis were careful about recalling traditions, especially those assigned to so major a figure as Rabbi Johanan.

Goodman gives slight attention to the Messianic fervor that, we may conjecture, played an important role in rousing the Jews. True, Josephus does not mention any Messianic leaders as such; but, in a passage (*War* 6.312) very significantly paralleled by both Tacitus (*Histories* 5.13.2) and Suetonius (*Vespasian* 4.5), he declares that what incited the Jews to war more than anything else was a prophecy, obviously Messianic in nature, that someone from their country would become ruler of the world.

Goodman gives scant attention to the role played by the “Palestinians”—the estimated one million non-Jews who were then living in Judaea. Surely, an important factor leading to the revolt is the high concentration of natives in the Roman army in Palestine (*Ant.* 19.357). The Roman failure to handle fairly the hostility between the Jewish and non-Jewish inhabitants of Caesarea was the immediate cause of the revolt. Goodman similarly gives little attention to the extraordinary success of Jewish proselyting activities, which threatened to undermine the spiritual foundations of the empire.

In sum, the revolt, in its desperation, was the result of many factors. Goodman is right in insisting that the data supplied by Josephus, despite his bias, remain the best guide to analyzing those

issues. The failure of the ruling class to maintain the confidence of the masses, as argued so persuasively by Goodman, undoubtedly played a key role, but other factors were no less important.

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KENNETH W. HARL. *Civic Coins and Civic Politics in the Roman East, A.D. 180–275*. (The Transformation of the Classical Heritage, number 12.) Berkeley and Los Angeles: University of California Press. 1987. Pp. viii, 253.

The history of the Roman empire in the third century rests on inadequate and unreliable literary sources; epigraphic and numismatic materials, thus extraordinarily important, have not always been studied in detail. The so-called Greek imperial coins, minted by the more or less autonomous Greek-type cities of the eastern empire, have received much less attention from scholars than have the main coinages of Rome. The neglect of the Greek imperial coinage relates to its seemingly uninteresting, often obsequious types as well as to its diversity and complexity: several hundred cities at one time or another—some of them for nearly three centuries—produced these coins, used for local circulation. Mostly bronze, but some in precious metals, the types glorify local gods, temples, officials, and festivals as well as the emperors and their victories.

This book does not attempt to analyze the whole corpus of this civic coinage but to present a selective survey, emphasizing what the coins indicate about urban change in the East during a time of troubles for the empire at large. The theses are convincing; the work is well-researched; it closely integrates new information with the general history of Rome and contributes to a better understanding of general history as well as of the Hellenized urban centers of the East. Introductory chapters and summaries, along with extensive notes and other apparatuses, including thirty-six plates (that are themselves a visual survey), make the book at least usable by nonspecialists.

Kenneth W. Harl speaks mostly of the ruling elite, the “Greek notables,” of the cities in question. The advent of the empire caused them to stress local loyalties and hold to the illusion of independence; they developed a strong loyalty to the empire, especially when the Parthian and Persian wars of the second and third centuries impressed on them the value of imperial protection. The cities were healthier during the first half of the third century than is usually realized: “the coins give little hint of a great third-century conflagration that consumed the classical city, its

elites, its life, and its gods. To the contrary, the coins preserve an unbroken record of expressions of civic patriotism, devotion to ancestral gods, and loyalty to Rome" (p. 83). Only later in the century did the local coinage flicker out, primarily because of inflation, at the same time that the notables in the cities of the East declined under the impact of imperial demands and external invasions. In the fourth century, the notables tended to be replaced as the elite by the Christian clergy, and not even Julian could reverse the trend.

The following are selections from a sizable list of complaints: one feels that at times Harl calls obsequiousness loyalty and sees more in the coins than is there. He exaggerates on occasion, seeing, for example (p. 89), "a rising sense of civic identity . . . [an] increased sense of civic identity" as late as 251. Is "sovereignty" (pp. 25, 27) the right word to describe the circumscribed autonomy granted to the Greek cities?

The volume is, nonetheless, well done overall, a valuable contribution to our knowledge of an uncertain period.

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H. D. RANKIN. *Celts and the Classical World*. Portland, Oreg.: Areopagitica or Croom Helm, London. 1987. Pp. 319. \$48.95.

This book is a compilation and an evaluation of descriptions by classical writers of the peoples known as Celts. Its purpose is "to observe the Celts through the eyes of the Greeks and Romans" (p. 1). Between an introduction and a conclusion, thirteen chapters are organized around chronology (five chapters cover the time from the founding of Massalia [Marseille] around 600 B.C. through the late Roman period), geography (on Iberia, Galatia, and Britain), and specific topics (classical sources as anthropology, Cisalpine literature, Celts in classical visual arts, Celtic women, and religion). H. D. Rankin shows how the classical sources can inform us about events involving Mediterranean peoples and Celts and about the character of Celtic peoples. Evidence from philology and place names is brought into the discussion, as is some archaeological data.

The book is valuable as a compilation and intelligent discussion of the classical sources. Particularly useful is the author's concern with trying to understand perceptions and attitudes of the classical writers regarding their northern neighbors. This synthesis is an excellent reference work for ancient historians, early medieval historians, archaeologists, classicists, and others interested in

the cultural interaction between classical and other peoples in the ancient world.

Methodologically this book is traditional. It makes relatively little use of the abundant archaeological data now available pertaining to interactions between Celtic groups and classical societies. The title of the book should include a subtitle such as "The Literary Evidence" to inform potential readers of the coverage. The present title implies that the treatment is broader than it is.

Rankin might have mentioned the rich theoretical literature on contact between societies, especially that pertaining to perceptions by one group of others. Intercultural contact and ethnic identification are important topics in anthropology today, and these issues should be noted in this study. Rankin interprets many statements by classical writers about Celtic groups quite literally; recent studies of cross-cultural perceptions suggest that we need to exercise critical caution in interpreting such information.

This new book is a valuable resource for scholars working on questions involving interactions between Europeans living in the temperate zone and classical peoples, but readers should be aware that it covers only a portion of the available information.

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MEDIEVAL

GEORGES DUBY, editor. *A History of Private Life*. Volume 2, *Revelations of the Medieval World*. Translated by ARTHUR GOLDHAMMER. Cambridge: Belknap Press of Harvard University Press. 1988. Pp. xiii, 650. \$39.50.

Medievalists are accustomed to setting their agenda in response to challenges from scholars of the early modern era. After all, the term "Middle Ages" itself was coined by the Italian humanists and their eighteenth-century epigones. By situating "the Renaissance" in the fourteenth century, Jacob Burckhardt attempted to define the modern age and distinguish it from the Middle Ages. This spurred medievalists to look for preceding renaissances with similar characteristics. By associating the emergence of capitalism with the Protestant reformers, Max Weber and R. H. Tawney also stimulated medievalists to examine antecedent commercial practices and economic theories. And, most recently, by identifying unique features of childhood, family life, and death in the early modern period, the late Philippe Ariès came to conclusions that prompted medievalists to put them to the test in the preceding period. When,

therefore, Ariès proposed a pioneering project of writing a comprehensive history of private life, Georges Duby, the eminent French medievalist, had the courage and sagacity to join up and later assume responsibility.

In this second volume devoted to the Middle Ages, Duby and his five French colleagues have, in fact, concentrated their efforts on France and Italy primarily in the periods of 1150–1220 and the fourteenth and fifteenth centuries. In a series of overlapping essays devoted to broad themes, which cannot avoid repetition and occasional contradiction, they have produced a loosely organized work. The most apparent division in the book, however, is between the earlier and the later periods. For the twelfth century, Duby, who treats public and private power and feelings of solitude, and Dominique Barthélemy, who studies lineage and marriage and castles and residences, outline broad thematic treatises that are strong on conceptual rigor but necessarily weak on illustrative detail. For the late Middle Ages, in contrast, Charles de La Roncière, who treats the Tuscan notables, Philippe Contamine, who describes French domestic architecture, and Philippe Braunstein, who explores the notion of intimacy, are able to produce vivid tableaux of private life assembled from a wealth of fascinating details furnished by written and visual sources. (Many illustrations are lavishly provided on the relevant pages.) Between these two sections stands Danielle Régnier-Bohler's chapter on imaginative literature. While the others often draw on literary sources for examples, she directly addresses the implications of looking to literature for historical documentation and elaborates specific literary themes that are relevant to society. Because of the richness of these materials, her discussion yields the most vivid details for the earlier period. Arthur Goldhammer's readable translation is adequate for the historical sections, but, unfortunately, he is not able to convey the full nuances and difficulties of Régnier-Bohler's critical theory.

Duby opens the volume with a semantic inquiry that defines private life by opposing it to public. This negative approach, encompassing all that is left over, might have degenerated into the clutter that has often passed for "daily life in the Middle Ages." Fortunately, he and his collaborators have attempted to articulate a limited number of controlling themes that impose overarching coherence on their heterogeneous materials. Four in particular are worthy of note: the house, the body, women, and individuality. Because private life is preeminently that of the family, the house (*domus*), ensuring immunity to the public world, occupies the center of private space. The authors begin with a division of this interior space (originally

between hall and chamber), proceed to its imbrication into multiple rooms, studies, and chapels for private use, but always end in the bedroom, the very heart of privacy. En route they trace the appearance of fireplaces, locks, shutters, and other furnishings but again conclude with the intimacy of the bed. Although society traditionally masked the body with clothes to endow social identification and gender differentiation, the body itself, formerly depreciated, came increasingly to the fore in the Middle Ages. Its beauty was depicted, praised (especially the hair), and enhanced through cosmetics and bathing both privately and communally. Its nudity was acknowledged, enjoyed, and explicitly associated with the sexual act that naturally reverted to the bed. The woman, for her part, was archetypically the private person of medieval society since she was excluded from a public role in most matters. All of the authors explore her space and activities but dwell principally on her functions in marriage, family, reproduction, and nurture of children. Individualism, finally, Burckhardt's old focal point, is now expanded to include a whole spectrum of associations that result in intimacy. Piety, not only that of the anchorites but also that of the laity, was increasingly individualized through private reading, prayer, and confession, all encouraged in the preaching of the friars. Literature in the first person not only stimulated egocentric autobiographers from Abelard to Petrarch but also memorialists and historians who placed themselves in the narration of their accounts. In romance the solitary knight-errant was obsessed with his identity and the self-conscious expression of prowess and love making. And painters, while improving their techniques, not only achieved greater realism but also individualized their portraits, intruded themselves as observers in their tableaux, and finally, like Dürer, turned to self-portraiture.

At the outset of the first volume, Duby warns the reader that he and his collaborators are like a team of archaeologists who have begun to make exploratory excavations into a rich mound but have not yet been able to lay bare its full contours nor the mass of its contents. At the end of the second, Philippe Braunstein contemplates "this most palpable pile of objects" and sums up: "No final interpretation, no irrefutable, definitive conclusion emerges. The search for the remains of intimacy is far from complete." But the French historians are to be lauded for their achievement. Without falling prey to a preoccupation with the origins of the modern world, they have inaugurated one of the great historical enterprises of our times and have set the agenda for succeeding generations.

The sumptuous layout and illustrations of the

French edition by Seuil have been fully replicated in the English version published by Harvard University Press.

JOHN W. BALDWIN
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WALTRAUT BLEIBER. *Das Frankreich der Merowinger*. Vienna: Böhlau. 1988. Pp. 194.

The year 1988 was an *annus mirabilis* for Frankish history. In addition to the book under review, Patrick J. Geary's *Before France and Germany: The Creation and Transformation of the Merovingian World* and Edward James's *The Franks* also appeared. James's book is the most wide-ranging of the three; Geary's is the most synthetic and interpretive. Waltraut Bleiber's book is marked by two qualities: it has the strongest narrative line—it is extremely detailed down to Dagobert I but quite thin thereafter—and it makes readily accessible current Marxist orthodoxy on the Merovingians.

There is no point in trying to summarize Bleiber's detailed, readable, and useful narrative of Merovingian history. Suffice it to say that, down to the mid-seventh century, this study is fuller and more intelligent than any other work currently available or, in my view, ever written. It seems well grounded in German literature but less so in French, English, and American scholarship. There are a few slips and some points that readers will take issue with, but, on balance, this is a remarkably successful political account.

Readers will almost certainly be more interested in Bleiber's account of Merovingian social development. She begins by saying that Merovingian history has been overlooked and that this is a bad thing precisely because the period was a crucial stage in the transition from a prefeudal to a feudal society. Bleiber envisions a "military democracy" at the opening of traceable Frankish history when leaders were obliged to listen to the voices of their followers. Leaders and followers were part of, and not sharply distinguished from, ordinary people engaged in a "primitive communal mode of production." In time, leaders had to choose the way of conquest in order to secure more lands with which to reward their followers. These conquests had two possibly unforeseen consequences: first, they put considerably more wealth than had ever before been accessible in the hands of an elite element in society and in doing so differentiated that elite from ordinary free men and women who were becoming increasingly subordinate economically, politically, and even legally; and, second, kings, at the head of conquering war bands, secured resources, largely at the expense of the disappearing Roman state apparatus, over which

their followers had no control. The result of these related processes was the creation of a class society wherein a great majority was forced to work and to produce a surplus for the benefit of a minority that did little or no work. Moreover, the top echelons of the land-holding elite, along with the kings, transformed the communal and consultative bases of prefeudal society into the royal courts and public assemblies of the dawning feudal age.

The argument of the book has a conceptual elegance that is beguiling, and it explains a great many problems in wholly plausible ways. But there is a rub. Bleiber proceeds by assertion and description more than by analysis and demonstration. The thin foundation of evidence will not often bear the weight of the interpretations she places on it. The "primitive community" is just the old *Markgenossenschaft* brought back in a new form, and those "military democracies" are pretty hard to find in the written sources. How so many previously free Franks (and others) wound up in subordinate positions is something every historian of the period would like to know, but our ignorance is not attributable to a lack of effort.

Conceptual issues aside, the book has some other shortcomings or, really, omissions. There is absolutely no account of intellectual life. Trade is skipped over in a few lines, and nothing much is said about cities. Religion and the church are neglected except as props to the monarchy and as means for extending agriculture and, with it, exploitation into the countryside. Still, this book raises fascinating issues in stimulating ways and brings forth valuable contributions not to be found in Geary's and James's works.

THOMAS F. X. NOBLE
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J. M. HUSSEY. *The Orthodox Church in the Byzantine Empire*. (Oxford History of the Christian Church.) New York: Clarendon Press of Oxford University Press. 1986. Pp. xx, 408. \$59.00.

At last there is a scholarly, sympathetic, readable, monograph-length history in English of the church of the Byzantine empire. Educated general readers and interested students, as well as scholars moving out of their narrow specialties, now have a place to go to learn about the history of the Byzantine church. The general reader will find here a clear treatment of a line of religious development different from that of the medieval West. The student of Byzantium will find here a much-needed study of the history of "the other institution" of the Eastern empire. The scholar will find a very useful presentation of the current state of

scholarship on Byzantine ecclesiastical history with excellent bibliographic aids.

This impressive work of J. M. Hussey's "retirement" chronicles the history of the Byzantine church from the monothelite controversy to the fall of the empire, that is, from the early seventh century to the mid-fifteenth century. The starting point, which is a bit eccentric for Byzantine specialists, is dictated by the series for which the volume is written. As a result the study is a history of the mature Byzantine church with only a brief introduction on the formative period. The treatment is largely institutional and biographical and focuses on Constantinople. Although one might have preferred more emphasis on the basic levels of church life, the capital of the empire and center of its church is consistently the best-documented region of the empire for ecclesiastical as well as for civil affairs. Thus, of necessity this work is history from the top down, but, as is the case in any truly popular institution, important local problems worked their way up to the top, where they were analyzed, handled, and documented.

The major part of the volume is titled "Challenge and Response within the Historical Framework," and this is the mode of presentation the author chooses. The Byzantine church, Hussey believes, evolved in large part by reacting to changing circumstances. Thus, the chronological chapters (of varying length) emphasize the church's redefinition of itself in response to catalytic events. The introductory chapter focuses on the Christological problems that differentiated the Byzantine church from the churches of the Semitic East. The second chapter, which treats the iconoclast controversy, emphasizes the process of intellectual and cultural self-definition that the Greek-speaking church underwent once it was ideologically out of communion with Semitic Christianity and politically out of contact with the Latin West. The following chapter, "Age of Photius," details Byzantine reaction to evolving changes in church and state in a western Europe now effectively outside the empire, and the following two chapters, which cover the period 886 to 1025, emphasize the working out of a new balance in church-state relations within the empire as the Byzantine church spread its power beyond the imperial borders into the newly converted Slavic lands of the Balkans and Rus'. The final three chapters of the chronological portion of the book treat the period from the death of Basil II in 1025 to the fall of Constantinople to the Ottomans in 1453, that is, the time of the Crusades and the Turkish invasions. Here Hussey's major theme appears most clearly: the disaffection between the Greek and Latin churches that had been growing since at least the seventh century eventually

doomed the possibility of Christian cooperation, cooperation that might have stopped the Muslim threat to Byzantium's continuance as a political entity. The book concludes with a short but very useful presentation of the basics of Byzantine church life, ecclesiastical administration, and popular spirituality, material that might have been integrated into the main part of the work.

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ANNA D. KARTSONIS. *Anastasis: The Making of an Image*. Princeton: Princeton University Press. 1986. Pp. xviii, 263. \$57.50.

The Anastasis in Byzantine art and literature signifies the Resurrection. Although it has often been confused with the descent of Christ into hell, or the "harrowing of hell," the Greek roots of the term indicate that it means both "rising" and "raising," the rising of Christ and his raising of the dead. The iconography of the scene as it came to be formulated depicts Christ's defeat of Hades, or Death, the overthrow of his kingdom, and the liberation of its prisoners, beginning with Adam and Eve. The reader looks in vain for some explanation of the persistence of the Homeric and pagan Greek word "Hades" in a Christian context, apparently taken over from the Septuagint as the Greek equivalent of the Hebrew "sheol." This remarkable book, however, is the first comprehensive analysis of the development of the iconography of the Anastasis by Byzantine artists, a theme central to Orthodox theology.

Anna D. Kartsonis summarizes and criticizes the deficiencies of the older and existing historical literature on the subject before proceeding to discuss the prehistory of the image, the pictorial evidence, and its interpretation in surviving written sources. She makes it clear that early Christian artists were reluctant to attempt the portrayal of Christ's death for fear of graphically misinterpreting the almost indefinable theological problems that the event imposed. Not until the eighth century, after the Sixth Ecumenical Council (680-81) and the Council in Trullo (692) had pronounced on some of those problems, were artists bold enough to depict the Crucifixion, the Entombment, and the Anastasis, and then they did so as a pictorial expression and defense of Orthodox christology against those who had been condemned as heterodox or heretics. In this development the *Hodegos*, or Guide-Book, of Anastasius Sinaites with its illustrations (late seventh century) was a crucial text, here minutely analyzed. Nonetheless, caution had still to be exercised. "Innovation" (*kainotomia*) in Byzantine Greek is another

word for heresy. The acceptable pictorial formulae for Christ's death and resurrection were not established all at once. The earliest extant representations of the Anastasis in its new form are all, as it happens, in Rome, dating from the pontificate of John VII (705–07), although there is enough evidence to show that the concept was Eastern in origin. Pictures of the Crucifixion and of the Entombment, however, were slower to enter the accepted canon of cycles of the life and death of Christ. While the earliest versions of the Anastasis revealed the christological aspect of the subject, later depictions concentrate on its soteriological message. By the end of the ninth century, the Anastasis had become the image for Easter, the most important feast in the church calendar.

This is a valuable study, handsomely produced and well illustrated. New books so ably marry art history to theology, sociology, and historical context as well as to the evolving tradition of doctrinal and liturgical practice in the church. The author carries her research no further than the twelfth century. She would surely agree, however, that the depiction of the Anastasis was brought to its triumphant conclusion two centuries later, as a masterpiece of visual, theological, and liturgical art in the apse of the side chapel of the Church of the Chora (Kariye Djami) in Constantinople.

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LAWRENCE NEES. *The Gundohinus Gospels*. (Medieval Academy Books, number 95.) Cambridge, Mass.: Medieval Academy of America. 1987. Pp. xiv, 263; 35 plates. \$40.00.

This book is a detailed and comprehensive study of an early medieval manuscript, one that stands at a dividing line in the evolution of the illuminated book and, indeed, of medieval literary culture. Named for its scribe and illuminator and preserved in the municipal library of Autun, it was written about A.D. 754 at an unidentified location probably not too far from where the manuscript remains to this day.

The manuscript contains the four Gospels in Latin, some ancillary texts, including twenty brief explanations of Gospel passages, and a series of miniatures: illustrated Eusebian canon tables, a picture of Christ in Majesty, and full-page representations of the four evangelists. The poor calligraphy, unclassical spelling, careless copying, and rudimentary Latin reflect the level of scribal culture at the end of the Merovingian age, supporting the view that it is an example of Merovingian barbarism and northern culture shock. But some

earlier commentators on the manuscript have differed in their assessment of its character, looking on it as an expressive and individualistic work of considerable brilliance.

Lawrence Nees provides a meticulous analysis of this early medieval book, its texts, scripts, and miniatures, based on a wide-ranging comparison with other manuscripts and with evidence in other media. It is his method not only to focus on the miniatures themselves but also to survey the components and characteristics of the manuscript in its totality. With this emphasis on the manuscript, such features as the state of the language and the explications of the Gospels are consciously excluded from the author's analysis. Most of the work, then, is devoted to a study of the portraits of the evangelists; the Christ in Majesty miniature, one of the earliest examples of this key image in the medieval West; the cycle of the miniatures in the codex (the first of its kind in medieval manuscripts); and the time and place of origin and the nature of the model on which the Merovingian manuscript is based. The book's thirty-five plates and seventy-one additional illustrations help the reader follow the author's arguments.

In the face of the evidence marshaled here by Nees, one is inclined to accept the unity of the manuscript: it was written and illuminated by a single author from a single source. One can also agree with Nees that the model of the Gundohinus Gospels was a lost manuscript of virtually identical content written about the middle of the sixth century at or near Ravenna. If this is so, the Merovingian codex supports the existence in Ravenna at that time of active writing centers and adds to our knowledge of Byzantine book illumination before iconoclasm. One may, however, find it difficult to accept Nees's idea that the peculiar order of the miniatures, the placement of the picture of Christ in Majesty at the beginning, and the placement of the four evangelists at the end were intended to give the book a magic property and protective function. In any event, the book written and illustrated by Gundohinus has important implications for the understanding of the "Carolingian Renaissance," especially the role of classical models and the existence of a central program in Carolingian book illumination, however much Gundohinus was inspired by private devotion and local interest.

Nees's book represents exhaustive research, and its theses are convincingly argued. It is also admirably written and exactly and beautifully produced.

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CHRISTIANE SCHUCHARD. *Die Deutschen an der päpstlichen Kurie im späten Mittelalter (1378–1447)*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 65.) Tübingen: Max Niemeyer. 1987. Pp. xv, 423. DM 108.

This book is meant to deal with three different problems: the history of the curia, relations between papacy and empire, and the social history of the city of Rome. The work is modeled on Bernard Guillemain's *La cour pontificale d'Avignon* (1962) and is based primarily on the entries published in the *Repertorium Germanicum*, a listing of all those individuals from German areas whose presence in Rome is attested in extant papal sources. Except for a few French-educated Germans from places such as Metz or Toul, Germans were not often found in Avignon. The creation of a second curia occasioned by the schism offered Germans a chance to increase their presence. The number of them in the curia jumped after 1378 and again in the postconciliar period under Martin V and Eugenius IV.

Like Guillemain, Christiane Schuchard distinguishes between the papal court, broadly defined (including all those merchants, petitioners, and functionaries who were described as "following the curia"), and the court, strictly defined (those in the pope's immediate circle, his *familia*, and cardinals). Germans seem to have been singularly underrepresented in the restricted circle surrounding the pope. They most often filled modest positions in the papal bureaucracy or in the households of cardinals. As foreigners in every sense, Germans held few important positions (there were few German cardinals and almost no influential ones). The problem was that normally important members of a household were recruited from the curia, because of friendship or personal service. Influential Germans were auditors in the Rota (usually French- or Italian-trained lawyers) or papal subdeacons (chosen for political reasons by Martin or Eugenius). Although the most influential Germans often were of noble background, most of the curialists were of urban origins and from the Rhineland. Schuchard's thorough analysis of the various Germans who served in Rome corrects on a variety of points work left incomplete by the late Clifford Maas. Most important is the makeup of the two German confraternities in fifteenth-century Rome. Schuchard believes that especially after 1444 they were dominated by clerics resident at the curia. In fact, to the extent that there was a German community in Rome, it centered on residence in houses rented from the German confraternity of Santa Maria dell'Anima in the neighborhood of Piazza Navona.

Readers will want to know the impact of the

Germans on the papacy, the city of Rome, and on the lands from which the curialists had come. At this level Schuchard's book is not helpful. She does not seem to keep in mind the three tasks she set for herself in the introduction. Unlike Guillemain's book on Avignon or even Maas's incomplete book, *The Germany Community in Renaissance Rome* (1981), Schuchard's work lacks a clear sense of a complete, integrated subject or thesis or what contribution an essentially prosopographical study can make. Perhaps because the biographies she collected on nearly twenty-five hundred persons are fragmentary, the direction of the book and the issues that need to be addressed are not always clear. One of the best sections of the book is a short discussion of Lübeck's need for representatives in Rome. At that point the importance of the issue of German influence becomes clear. In an ironical sense, curialists were important to their friends and relatives in Germany because those who did belong to a cardinal's household might be able to provide an introduction or access to someone who really was important. The first cardinal to represent imperial interests in Rome was not German but Italian, Aeneas Sylvius Piccolomini.

One hopes that Schuchard will continue her work on papal-imperial relations and on how these relations were affected by the modest presence of Germans at the curia.

DUANE J. OSHEIM
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DAVID NICHOLAS. *The Metamorphosis of a Medieval City: Ghent in the Age of the Artevelde, 1302–1390*. Lincoln: University of Nebraska Press. 1987. Pp. x, 369. \$35.00.

Except for Paris, Ghent was the largest city north of the Alps in the late Middle Ages, and David Nicholas has for some time been the leading English-speaking student of its history. For readers who know their Pirenne, or who are familiar with more recent literature in Dutch, Ghent is best known for the bitter and recurring social conflicts in which embattled weavers sought to carve out dominion for themselves, overthrowing the patrician drapers and (at times) subjugating plebeian fullers. By contrast, Nicholas's Ghent is less catastrophic in its historical development and less dependent on the fortunes of its woolen industry.

Nicholas's central thesis is that, as its cloth industry declined precipitously in the last third of the fourteenth century, Ghent took on a new economic identity as a regional market center, especially for the reconsignment of grain and other French goods farther east and north along the Scheldt. Part of this argument calls for evalu-

ation by demographers, since it depends on population figures for the late fourteenth century that are lower than those given by other scholars—which, given various consumption indexes, means that the remaining Gentenaars had a higher living standard than has been thought. In other respects the argument seems clear and compelling: population declined severely by any measure, but since membership in the city's many small guilds remained constant or nearly so, one has to assume the textile sector suffered a disproportionate loss. Although rates for city assizes are not always known, it is also surely indicative of a trend that receipts for a tax related to the preparation of export-quality cloth went down 80 percent, while during the same period excises on food and drink and contracts for the farm on the Dendermonde toll (further down the Scheldt) increased dramatically. Finally, it is at least congruent with this picture of Ghent that the small guilds were the one "member" of the city that was always represented in the governing councils, while other corporate players in the urban political game—weavers, fullers, and "poorters," or established burghers—came and went depending on circumstances.

Along the way, Nicholas exploits court records and other sources for interesting insights into the economic life of the city—for instance, indications that wealthy Gentenaars kept their money invested in one way or another and kept little ready cash on hand. There is, however, a trees-and-forest problem, as is often the case with good local history. By my count the book has fifty-nine tables and graphs, most of which are pertinent to the argument or at least interesting, but in some cases (for example, the percentage of cases dealt with by the aldermen in which there is reference to foreign trade [p. 179]) one senses the wisdom of the maxim that not everything that can be quantified needs to be. Sometimes in commenting on a particular case or bit of information, the author draws inferences that seem unclear or elliptical, as if stating a premise that would be obvious to specialists on Ghent but not to others.

More important, his focus on economic and social relations tends to obscure the impact of dramatic political events. He does point out the devastating effects of the civil war of 1379–85, but the "guild revolution" of 1302 has no significance for the argument of the book except as a *terminus a quo*. In fact, the merchant-draper, overthrown in 1302 along with the patrician government, are a missing presence throughout the book, particularly when the author describes the rather haphazard way finished cloth was marketed or notes that weavers' guilds did not see the potential for exporting medium grades of cloth. The Great Pirenne, as he is rightly called here, does indeed

require revision at the hands of scholars who know the sources as well as Nicholas does, but he also had a fine sense for the interdependence of politics and economics.

JAMES D. TRACY
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JULIET R. V. BARKER. *The Tournament in England, 1100–1400*. Wolfeboro, N.H.: Boydell. 1986. Pp. 206. \$40.00.

This book is the first to analyze the tournament primarily as sport. Employing both literary and historical sources, Juliet R. V. Barker examines the development of the tournament from its origins at the turn of the twelfth century to the turn of the fifteenth. Barker places the tournament in its military, political, and ecclesiastical contexts; she emphasizes the impact of technological developments on the techniques of tourneying, the role of spectators, and the tournament as preparation for war. The author discusses with care the tournament as a political phenomenon: its danger to civic stability when the king was weak, its role as political propaganda for the ruler when the king was strong. Strong monarchs controlled tournaments, weak ones feared them. Other historians—for example, Richard Barber, Malcolm Vale, and Maurice Keen—have devoted attention to the sportive aspect of these war games, but Barker concentrates on this aspect. Her discussion of more serious, broader aspects of the tournament should be supplemented by Richard Kaeuper's *War, Justice, and Public Order: England and France in the Later Middle Ages* (1988, pp. 184–268), an imaginative and thoughtful study that finds tournaments and chivalry less significant as sport than as threats to public order.

Tournaments, which originated in France, had become sufficiently numerous by 1130 that the church prohibited them, with about as much success as the contemporary Roman Catholic Church prohibits birth control. Barker finds the earliest mention of tournaments in chivalric literature of the second half of the twelfth century (p. 6). She deftly traces the evolution of the *hastiludium* from a relatively disorganized and often lethal brawl into the regulated (both by the king and by its own officials) courtly spectacle of the later Angevin period. The author's stress on the tournament as an integral part of chivalric culture, although hardly novel, is welcome and helps place tourneying in its own milieu. Barker also stresses not only the international nature of chivalric culture but also the relation of tourneying to the rise of nationalism.

Barker's sterling book is clearly written and

logically organized. Its scholarship is à *outrance*, its appeal to the student of the chivalric period à *plaisance*.

JAMES W. ALEXANDER
University of Georgia

CHRIS GIVEN-WILSON. *The English Nobility in the Late Middle Ages: The Fourteenth-Century Political Community*. New York: Routledge and Kegan Paul. 1987. Pp. xxii, 222. \$39.50.

Chris Given-Wilson has written a valuable survey of an important field of study. Although he restricts himself, a little arbitrarily, to the fourteenth century, he makes amends by adopting a broad definition of "nobility" embracing not only the peerage but also the gentry.

The first part of the book is concerned with the ranks of nobility and includes chapters on the titled nobility, the peerage, and the gentry. In the first chapter, the author usefully adopts a chronological framework, carefully analyzing the relations of successive kings with the aristocracy and documenting the gradual inflation of honors. The theme of increasing stratification of the peerage is pursued in the second chapter, stratification increasingly defined by an individual summons to Parliament. Recruitment to the baronage was almost entirely hereditary or otherwise linked to the acquisition of baronial estates. Given-Wilson shows that the few lords summoned by the king on their personal merit were set apart as "bannerets." The third chapter is an important synthesis of recent work on the gentry. Although the author is perhaps too schematic in his distinction between "country" and "parish" gentry (especially in view of the enormous differences in wealth, power, and life style in different parts of England), he is admirably clearheaded in his survey of work on gentry communities and gentry power.

The second part of the book is more thematic, with important chapters titled "Households and Councils," "Estates in Land," and "Property, the Family and Money." Perhaps inevitably, there are some problems with the study, especially unevenness and overlap. The author's discussion of households relies almost entirely on material about the peerage. His study of estates, though the best available, is clearly premature, and his view that smaller landlords were hardest hit economically in the fourteenth-century crisis may well soon stand in need of modification. The third chapter is rather unwieldy and curiously neglects forms of careerism other than soldiering.

All in all, however, Given-Wilson has produced an impressive study. The quality and range of his introduction makes me wish that he had extended

the scope of the book to treat more fully the cultural and religious concerns of the nobility. He has left student and teacher greatly in his debt and provided sound conceptual foundations for the future research he will almost certainly stimulate.

MICHAEL J. BENNETT
University of Tasmania

JOHN TAYLOR. *English Historical Literature in the Fourteenth Century*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xii, 349. \$59.00.

In the preface to this book, John Taylor describes the intent of his work modestly and succinctly: "The following study attempts to illustrate the principal features of the historical literature of fourteenth-century England." For a sentence written so recently it has a curious ring of nineteenth-century learnedness. Something in the sedate and composed tone of "illustrate the principal features" carries echoes of Victorian botany, philology, entomology. And it is a work in that very respectable tradition—careful and patient research demonstrated without showiness, exact fulfillment of reasonable promises, scrupulously modest assertions, nothing outré, nothing to excess. Taylor refers to himself as "the author" even when expressing gratitude to his typists, his teachers, and his wife.

His use of the word "literature" in the title may be inadvertently misleading in these days when the word suggests such a wealth of definitions and critical instruments adapted from poetics, narrative theory, rhetoric, semiotics, and more. By historical literature, Taylor means "a continuous spectrum of 'historical writing'" (p. 39) that includes chronicles and annals, registers and cartularies, letters, poems, in short, any writing that might contain reliable information about events in fourteenth-century England. A useful distinction for describing studies of historical writing is that between source analysis and discourse analysis; source analysis examines the correspondence between a text and the world of events it purports to describe, and discourse analysis is directed toward narrative structure, rhetoric, organizing symbols and tropes, narrative voice, all the elements that combine to make a text a structure of linguistic meaning. Taylor is definitely a source man.

The book is organized into chapters on the general types of fourteenth-century historical writings: chronicles (St. Albans, Westminster), universal histories and compendiums, the French *Brut*, the *Anonimale chronicle*, letters, and political poems (including *Piers Plowman*). Some of the chapters are not strictly generic, and "chivalrous

history," including the works of Froissart and Jean le Bel and Thomas Grey's *Scalachronica*, is given a separate chapter on grounds of provenance (works written outside England) and content (knightly deeds and chivalric values). Similarly, narrative accounts of the deposition of Richard II are grouped together in chapter 9 and descriptions of Parliament in chapter 10. In every case, the "principal features" described are authorship (identification, career, political and institutional allegiances, access to books and records), production and distribution of manuscripts, and historical content in terms of scope and reliability.

Taylor is painstaking and thorough and straightforward in coping with factual matters. He becomes less comfortable, and less satisfying, the further he gets from those aspects of historical writing for which source analysis is an adequate approach. He treats poetry to exactly the same approach as annals—stating, for example, "The historical information of 'Durham Field' is scarcely helpful" (p. 253)—and he is rather at a loss to find anything much to say about the overwhelming popularity of the *Brut* legends, the persistence of Arthurian material, and the overlap between romance and history, except that people did seem to like that sort of thing. His traditional approach in both organization and method tends to break the material into discrete segments and prevents themes from emerging and unifying the book, although, from chapter to chapter, chivalric ideals and yearnings toward a romantic and heroic secular past seem to ask for a different kind of treatment. As it is, Taylor's book will, I think, be consulted more often than read.

The relation between Taylor's study and volume 2 of Antonia Gransden's *Historical Writing in England* (1982) has to be considered. Students of fourteenth-century historiography, using both books, will find that Taylor's study works as an elaborate and useful addendum to the fourteenth-century sections of Gransden's volume. Thus, where Gransden gives four or five pages to the *Brut*, Taylor expands the detail of manuscript and textual history to twenty-one pages. Where Gransden surveys sources for the entire reign of Richard II in thirty-six pages, Taylor concentrates on accounts of the deposition in twenty pages. And Taylor apparently considered that Gransden had sufficiently commented on the *Scalachronica*, because his account is briefer than hers. Taylor's book thus "fits" with Gransden's to rational purpose, although one has to turn to Gransden in every case for any sense of the tone, color, and idiosyncrasy of a work.

Taylor's survey is not about historical literature or even writing considered as writing; it is a scrupulous account of various written records as-

essed for their value as sources for a history definitely outside any text. The majority of his scholarly references are to works of seasoned vintage by his distinguished countrymen and women. His own writing is lucid, reticent, and unvoiced.

NANCY F. PARTNER
McGill University

MODERN EUROPE

ALISTER MCGRATH. *The Intellectual Origins of the European Reformation*. New York: Basil Blackwell. 1987. Pp. vi, 223. \$39.95.

Reformation studies are very much alive and well these days, though in contrast to a generation ago, when a theological view of things dominated, many of the important contributions nowadays come from scholars bent on "historicizing" the Reformation, that is, seeing the Reformation as a social and political rather than as a theological event. Here we have an exception to the rule, a creative and forceful reiteration of theological and intellectual perspectives. Alister McGrath, from whose pen we have monographs on the history of the doctrine of justification and Martin Luther's "theology of the cross," presents a sophisticated and exciting, yet terribly traditional, study in the history of ideas, although he acknowledges that there were also political and social origins to the Reformation (which he, in a similarly traditional vein, describes as nationalism, the growing power of the German cities, and so forth), but he confines himself here to an explanation of its intellectual origins. The analysis is learned and thoughtful. The conversancy with secondary literature is impressive, and primary sources are handled with insight.

McGrath's argument is embodied in the three opening chapters that survey late medieval theological thought, humanism, and scholasticism and in five additional chapters (with the heading "Sources and Methods") that trace the changes in attitudes toward theological sources and methods in the Reformation. McGrath shows (convincingly, in my opinion) that the period preceding the Reformation was characterized by the presence of two movements—Christian humanism and scholasticism—that were far more complex than has traditionally been assumed. There existed excessive theological diversity in the late medieval period, but there were no authoritative pronouncements from the church as to what was proper teaching (which is understandable in light of the fateful weakening of the *magisterium* in the Great Schism). This meant that theological opinion became confused with church dogma. Moreover, the

traditional use of Scripture in theology was bound to become problematic as soon as the new philological and literary techniques of humanism made inroads into theology.

McGrath argues that, even as the Reformation had two major expressions (Luther and Huldrych Zwingli/John Calvin), so did it have two intellectual origins—scholasticism in the case of Luther, humanism for Zwingli and Calvin. These expressions entailed two quite different objectives: justification and “a gracious God” for Luther, piety and reform of church and society for Zwingli and Calvin.

McGrath emphasizes that the theologies of the Reformation must be seen in the context of the late fifteenth century. Luther’s “evangelical discovery” was, in that context, a development within, rather than a categorical break with, late medieval thought. Zwingli, on the other hand, never wrestled with scholastic thought but, convinced that religion concerns life in all its fullness, was in continuity with humanism.

There are a number of loose ends in the argument. Not all of McGrath’s thesis is novel. Josef Lortz, for example, propounded the “uncertainty principle” for the fifteenth-century scene with great vigor, and in some respects McGrath reverts to earlier scholarship (regarding Zwingli’s humanism, for example, he is much closer to Walther Köhler than to Gottfried Locher). There is also the question of the intellectual origins of the other major stream of the Reformation, variously called the “Left Wing” or the “Radical” Reformation. Thomas Müntzer is not mentioned by McGrath at all, and Andreas Carlstadt is discussed only in relation to Luther’s development between 1515 and 1517. If the “radicals” of the Reformation deserve to be seen as a major alternative to Luther, Zwingli, and Calvin, some reflections would have been in order, particularly in light of the fact that the recent historiography of the Radical Reformation has emphasized the medieval sources of Anabaptism.

Despite such weaknesses, this is one of the best monographs on the Reformation in recent years.

HANS J. HILLERBRAND
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MARGARET C. JACOB. *The Cultural Meaning of the Scientific Revolution*. (New Perspectives on European History.) New York: Alfred A. Knopf. 1988. Pp. xii, 274. \$8.00.

In this study of how Newtonian science became an integral part of European culture during the seventeenth and eighteenth centuries, Margaret C. Jacob summarizes several familiar themes from

her earlier works on the English Newtonians and the radical Enlightenment. She also develops her new account of the relationship between the Scientific Revolution and the Industrial Revolution, a relationship that she argues is more straightforward than historians of science and technology have previously thought. Readers acquainted with Jacob’s other books will find that she has now extended her thesis concerning the role of science in the culture of lay elites in England, France, and the Netherlands, in order to describe how those lay elites transformed the uses of capital and the modes of production during successive periods of industrialization. Among other things, she tries to show that the delay in the occurrence of the Industrial Revolution in France and the Netherlands, as compared with its occurrence in England prior to 1800, had less to do with access to capital, cheap labor, water, and steam power than with the ability of entrepreneurs to think mechanically. The French and Dutch had access to capital and all of the modes of production, but “they simply could not have understood the mechanical principles necessary to implement a sophisticated assault on the hand manufacturing process” (p. 181).

Jacob’s interpretation of why the Industrial Revolution happened earlier in England depends on a sharp distinction between what she calls “the penetration of scientific and mechanical knowledge” among the educated public and the initial formulation of that knowledge among natural philosophers of the seventeenth century. Why did France and the Netherlands industrialize after England? They did so largely because their lay elites failed to assimilate as quickly as did the English the cultural consequences of Newtonian science. This failure did not mean that the French and the Dutch produced no great scientists during the period. Jacob recognizes the importance of René Descartes, Pierre Gassendi, and Isaac Beeckman, for instance, to the new science. Nonetheless, she singles out the English as constituting a society ripe for industrialization, one in which the gentlemen who attended lectures in Newtonian applied mechanics during the 1720s reared the sons who were already investing heavily in industry by the 1760s.

Although this account of comparative industrialization is clearly articulated, it may seem puzzling to the reader in view of the other half of Jacob’s thesis. In her earlier chapters, she describes the initial formulation of what she regards as the chief components of the new science, Descartes’s natural philosophy and Newton’s physics. Crucial to these chapters is the assertion that the cultural contexts of both England and the Continent were favorable to the development of science in the seventeenth century. In England, for exam-

ple, moderate Puritans and liberal Anglicans alike realized that the new physics was a means of restoring order to a political culture torn apart by the English revolution. The congruence of scientific beliefs and religious beliefs and the common political aims of scientists and educated gentlemen nurtured the new science in a culture already predisposed to accept its principles. Presumably, France and the Netherlands nurtured the respective principles of Descartes and Beeckman in comparable—albeit non-Anglican—ways. If Jacob's account of the lag in French and Dutch industrialization is correct, however, eighteenth-century French and Dutch cultures must notably have lacked an adequate “penetration of scientific and mechanical knowledge” despite the fact that they had, in the seventeenth century, successfully nurtured Cartesian science. Do the two halves of Jacob's thesis contradict each other? Or do they underscore genuine and paradoxical facts about the relations of science and industrialization? These questions are not dealt with in the present study, but they would be worth raising in future discussions of Jacob's work. It may be that, to avoid the contradiction or to resolve the paradox, historians like Jacob who seek to describe the cultural contexts of the new science will have to look more closely at what exactly constitutes a cultural context.

LYNN S. JOY
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JAMES C. RILEY. *The Eighteenth-Century Campaign to Avoid Disease*. New York: St. Martin's. 1987. Pp. xvii, 213. \$29.95.

In this volume James C. Riley argues that measures implemented during the eighteenth century by public health advocates (“environmentalists” [p. 137]) contributed to the decline of Europe's death rate, even though the true cause of disease was not understood. The author states that, whereas Europeans had previously adopted a passive fatalism toward epidemics, the Enlightenment doctrine of progress created fertile soil for more medical activism. A new determination to prevent and avoid disease grew out of the revival of the Hippocratic belief that habitat and environment could affect health. What Michel Foucault calls the gaze of the physician thus shifted away from individual care toward the greater social challenge of protecting whole populations by subjecting nature to human control through “drainage, lavation, ventilation and reinterment” (p. xiii). For it was the shared belief of doctors, according to this study, that environmental and atmospheric “constitutions” caused disease. Thus, medical topogra-

phies—tabulations of observations on places, climates, and disease—were expected to reveal meaningful patterns. And these patterns would enable doctors to render habitats less harmful and humans less vulnerable. Without discerning, then, the actual pathogens that cause disease, without even imagining the existence of microorganisms or comprehending the role of their vectors, the environmentalists launched an attack on standing water, fetid air, filth, and improperly buried corpses that inadvertently decreased the arthropod and rodent populations. The result was better health. Ironically, a misunderstanding of the cause of disease led to many salubrious measures being taken, albeit for the wrong reasons.

The basic argument here will come as no surprise to anyone familiar with the work of George Rosen or Richard Shryock. The compatibility between environmental engineering and the Enlightenment theory of progress has been noticed before by numerous historians of science, of medicine, and of Old Regime France (Charles C. Gillispie, Camille Bloch, Paul Delaunay, Daniel Roche, to name only a few). This book is written in a labored style and is quite repetitious. The language is frequently clumsy and sexist (populations are referred to as “aggregate man”). The jacket bears a grossly inaccurate French title for the cover picture. In general, the entire work should have been edited much more carefully.

Nonetheless, two things strike me as worthwhile. First, the environmentalist argument is rehabilitated, and this is an important correction to the histories of disease that stress contagionism as the only right theory. The author explains the inadequacies of the argument for contagion in its eighteenth-century form. Second, there is an inquiry into the related question of why the environmental hypothesis met with so little skepticism from physicians. It was untested, unconfirmed, yet it was embraced. This was partly a result of the lack of counter evidence, partly the poverty of statistical methods, and partly the eighteenth-century habit of mind that accepted analogical reasoning and the amassing of observations as paths to truth. The fact that no simple patterns emerged from the data, that no easy correlations appeared between places, weather conditions, and epidemics, did not lead environmentalists to doubt that such patterns and correlations existed. This absence of any critical scrutiny of their theory is nicely explained.

NINA R. GELBART
Occidental College

ROBERT GILDEA. *Barricades and Borders: Europe, 1800–1914*. (The Short Oxford History of the

Modern World.) New York: Oxford University Press. 1987. Pp. xvi, 498. Cloth \$58.00, paper \$19.95.

This is a competent, conscientious, but ultimately disappointing survey of nineteenth-century European history. The tone is largely factual, the coverage—given constraints of space—in many respects thorough. Robert Gildea's greatest achievement is the production of a book that will serve as a convenient reference for many students of modern European history on a number of key topics with a useful bibliography included.

The book divides into three chronological chunks, 1800 to 1850, 1850 to 1880, and 1880 to the outbreak of war. Gildea begins each segment by setting the demographic and economic framework with appropriate discussions of growing commercialization, then later the industrial revolution and urbanization, as well as shifts in agriculture, trade patterns, and population structure. Several chapters on major political and diplomatic developments follow, treading largely familiar paths but providing a good standard map. Chapters on culture begin with discussions of general cultural conditions, particularly educational developments but also leisure, religion, and in some cases other aspects of life such as prisons and poor relief; the more conventional surveys of art, science, and philosophy follow.

Gildea disclaims attempts at originality, and the book does not pursue novel interpretive themes overall. Thus, reaction dominates the 1850s just as it did in the series edited by William Langer, while the new stresses of socialism and minority nationalisms foul things up later on. There are, however, a few sections slightly more venturesome. A very useful chapter on politics after the 1880s uses new state functions, rather than partisan battles, as its main initial theme, focusing on education and welfare (though one would like more on the military).

For all its competence, however, the survey misfires somewhat. There is too little integration. This deficiency is a function in part of the avoidance of interpretive risk, but it also follows from a failure to integrate social-economic history with political history. Separate chapters remain separate; there is no interweaving of causation or impact. Even individual chapters, notably those on culture, that try some of the newer social history fail to pull together. Thus, in one case we move from prisons to romanticism with no pretense of connection save chronological concurrence. Culture, here, risks being a shopping list of items that do not fit an economic framework or political narrative. The author pays scant attention to social

class beneath the elite, which limits his ability to find integrative vehicles.

Efforts to discuss Eastern as well as Western Europe also misfire to a degree, and Eastern Europe continues to receive too little attention and too few appropriate conceptual frameworks. It in fact pops up mainly in brief narrative sections, with little treatment in chapters dealing with culture. Finally, partly because of the lack of integrating mechanisms, there are also too many topics barely covered. This is an inevitable lament in dealing with a survey, and I offer it with due hesitation. My sense, for example, that science is somewhat slighted (it is by no means ignored) may reflect individual taste. Nevertheless, the absence of attention to family history and women's lives strikes me as astonishing. If the current American passion for testing histories for women's content can be overdone, the field certainly has opened widely for this period of European history, yet Gildea, save in a brief passage on feminism, leaves it virtually untouched. Even leisure, which is noted as a promising newcomer, does not gain much attention, with many key issues unevoked. This kind of topical unevenness makes the book not only needlessly incomplete but also slightly old-fashioned despite the careful socioeconomic frameworks established for each time period.

It remains good to see new survey work in a field somewhat neglected in this respect in recent years; the result will aid serious students by acquainting them with considerable new work and by providing a solid if conventional narrative thread. This is not, however, a book that really tackles the key problems of interrelating new topics and old, and to this extent it falls short of the most exciting challenge.

PETER N. STEARNS
Carnegie Mellon University

LLOYD BONFIELD *et al.*, editors. *The World We Have Gained: Histories of Population and Social Culture; Essays Presented to Peter Laslett on His Seventieth Birthday*. New York: Basil Blackwell. 1986. Pp. x, 421. \$60.00.

It is not always easy to characterize a *festschrift*, but this volume proved easier than most. Many such collections lack a clear focus. Fortunately, this work does have a genuine organizing point: the demographic history of England and the continuing work of the students and researchers at the Cambridge Group for the History of Population and Social Structure. The book consists of fifteen essays, and of the seventeen contributors, five are former doctoral students of Peter Laslett's, four are coauthors, and eight are colleagues and co-

workers, many from the Cambridge Group. The title is an obvious play on words. Peter Laslett's provocative *The World We have Lost* was first published in 1965. Since then, as the preface to this volume points out, a great deal has been gained by his books, papers and essays, stimulating ideas, guidance of doctoral students, collaboration with colleagues, and encouragement in general of work in the field of historical social and demographic studies. Among other things, Laslett must be remembered as a co-founder (with Anthony Wrigley) of the Cambridge Group.

The social and demographic history of England provides the basis for twelve of the fifteen essays. Of the other three essays, one is on France in the twentieth century, one concerns the Italian Alps from the seventeenth to the twentieth centuries, and one is a general account of the development of statistical studies and simulation in historical demography connected to the Cambridge Group. The secular scope ranges from the Middle Ages to the 1970s. The papers are arranged in rough chronological order.

Although it is not always best form to provide a lengthy list of the contents of a collection of essays, it seems appropriate in this case. In order, the essays are as follows: "Population Turnover in Medieval Essex: The Evidence of Some Early-Fourteenth-Century Tithing Lists" by L. R. Poos; "Family Patterns and Relations of Bishop's Lynn Will-makers in the Fourteenth Century" by Jacques Beaurroy; "Marriage Processes in the English Past: Some Continuities" by R. M. Smith; "Illegitimacy and the European Marriage Pattern: Comparative Evidence from the Alpine Area" by Pier Paolo Viazzo; "Widows in Late Elizabethan London: Remarriage, Economic Opportunity and Family Orientations" by Vivien Brodsky; "Normative Rules and Property Transmission: Reflections on the Link between Marriage and Inheritance in Early Modern England" by Lloyd Bonfield; "The Social Order of Early Modern England: Three Approaches" by Keith Wrightson; "The Proximate Determinants of Marital Fertility in England 1600-1799" by Chris Wilson; "Did the Mothers Really Die: Three Centuries of Maternal Mortality in 'The World We have Lost'" by Roger Schofield; "Work, Welfare and the Family: An Illustration of the Adaptive Family Economy" by Richard Wall; "Men on the Land and Men in the Countryside: Employment in Agriculture in Early-Nineteenth-Century England" by E. A. Wrigley; "The First Scientific Social Structure of Modern Britain 1875-1883" by S. R. S. Szreter; "Welfare and the Historians" by David Thomson; "Mountains, Rivers and the Family: Comments on a Map from the 1975 French Census" by Herve Le Bras and Emmanuel Todd; and

"The Genesis of Experimental History" by Kenneth W. Wachter and Eugene A. Hammel.

For the student of English population history and historical demography in general, this book will certainly be worth a look. A number of papers are of sufficient quality to have easily merited publication in very good journals. It is unfortunate that papers published in such volumes tend to be less accessible than those appearing in journals. Thus, libraries should be encouraged to acquire this book. It is unlikely that all of the papers will be valuable to all readers, but some (such as those by Wilson, Smith, Wrightson, Wrigley, and Schofield) will likely be more widely cited. As examples, the essay by Wilson applies John Bongaarts's model of the proximate determinants of marital fertility to sixteen English family reconstitution studies for the seventeenth and eighteenth centuries to examine the sources of the relatively low marital fertility characteristic of pre-industrial England. Schofield estimates maternal mortality rates for some English parishes for the period 1550-1849, prior to the era of vital statistics. Wall provides an example of the use of census listings to study the family economy. The essay by Wachter and Hammel yields an interesting glimpse into the history of demographic simulation at the Cambridge Group.

In general, this volume represents a valuable addition to the series of publications that have resulted from the research of the Cambridge Group. As such, it warrants the attention of all readers who follow the work of that excellent institution.

MICHAEL R. HAINES
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J. R. SMITH. *The Speckled Monster: Smallpox in England, 1670-1970, with Particular Reference to Essex*. Chelmsford: Essex Record Office. 1987. Pp. 217. £14.95.

For decades, accounts concerning the conquest of smallpox in England have appeared with remarkable regularity, from Cyril Dixon's comprehensive description, *Smallpox* (1962), to Donald Hopkins's *Princes and Peasants* (1983). The topic has routinely attracted both historians and physicians. Studies of the dramatic saga of smallpox inoculation—its transfer from Turkish harems to the English court, the discovery of milkmaid's cowpox, and the preparation of a vaccine by Edward Jenner—all have found welcome places in Whiggish treatments of medical history.

J. R. Smith's work, however, is somewhat different. Smith attempts to review the well-known events within the narrow scope of one English county, Essex, using the rich resources of its

record office, documents from circa 1670 to the present. He has furnished us with valuable local data culled from its extensive collections. This is a praiseworthy effort because it allows readers to get away from dubious generalizations—including the writings of Peter Razzell—and actually concentrate on events in a predominantly rural county that pioneered smallpox inoculation during the second half of the eighteenth century.

Smith begins with an appraisal of the cultural and economic impact of smallpox on Essex in the 1700s. Dread and fear of the disease—then the most prominent health hazard after the retreat of plague from Western Europe—prompted hasty nocturnal burials and a ban on travelers. Damage to trade was considerable because markets were closed, an economic disaster compounded by payments to the sick and poor. Administrative, legal, and educational services were severely disrupted during epidemics. Having survived an attack of the disease paradoxically provided advantages for adults who sought employment in well-to-do households as servants and journeymen. Disfigured, often partially blind, they had paid their dues to the “speckled monster” and had become attractive bastions of resistance to the cyclical arrival of a dreaded scourge.

Subsequent chapters deal with the shifting patterns of smallpox inoculation, from its high-society adoption in the 1720s to its decline in the 1730s and reemployment in the 1740s. An increase in popularity resulted after the great epidemic of 1751–53 and the establishment of a simplified procedure by Robert Sutton and his son Daniel. Here the Essex evidence points toward the gradual adoption of mass inoculations for the poor, leaving only the newly born with a susceptibility to the disease. Indeed, Smith’s documentation of successful general inoculations in small and medium-sized towns of southeastern England, which clearly reduced the mortality from the disease, is one of the most important findings of this book. The author, however, is careful to point out that such effects merely contributed to the availability of potential marriage partners and parents, thereby supporting the Wrigley-Schofield thesis of eighteenth-century population growth.

The story then moves on to the transitional years 1798–1840 in which smallpox inoculation and cowpox vaccination competed for popular acceptance until the former was outlawed by an act of Parliament. The numerous vicissitudes of vaccination are then recounted. Smith is to be congratulated for his microstudy. As he suggests, similar research on other English regions may allow comparative analyses and eventually lead to

a much better understanding of society’s response to a very deadly disease.

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MORDECHAI FEINGOLD. *The Mathematicians’ Apprenticeship: Science, Universities, and Society in England, 1560–1640*. New York: Cambridge University Press. 1984. Pp. viii, 248.

This study reacts against a view implicitly accepted by many intellectual historians, namely, that the universities of Oxford and Cambridge contributed little or nothing to the Scientific Revolution in England at the turn of the seventeenth century. Mordechai Feingold argues against that thesis in a twofold way: first, by showing that it underestimates the time required for new science to be accepted among responsible scholars and second, by showing that solid bases for the reception of new ideas had been made at both universities under their Elizabethan statutes, beginning around 1560. Feingold terminates his survey roughly at 1640 because by that time the ideas of Galileo, Francis Bacon, Johannes Kepler, and René Descartes had been disseminated in England and the new era had definitively begun. The focus of Feingold’s interest is mainly, though not exclusively, Copernican, “the marriage between mathematics and astronomy” (p. 7). This focus explains his somewhat esoteric title: the apprenticeship for mathematicians to which he alludes is the long incubatory period that had to precede the ferment of activity in which they would be engaged by the mid-seventeenth century.

The project, though easy to state, proves not so easy to carry out. To make his point Feingold studies the more important figures of his period and many others lesser known; he pores over the university statutes; he evaluates the teaching community and the student community at both institutions; he characterizes the “scientific community” of the time; he looks into Gresham College and its role in the genesis of “London science”; and he ends with a survey of patronage, noting an ambivalent attitude toward sciences as theoretical as mathematics. His conclusions are perforce nuanced: obviously it would be anachronistic to expect to find the science of the 1640s already full blown in the 1560s; with proper qualifications the universities did lay the groundwork for the coming revolution; their mathematical instruction throughout the period shows evidence of becoming increasingly professional; and the drainage of their better people to London can be interpreted

along pragmatic lines, not seen as a result of intellectual bias against scientific inquiry.

In this, his first major publication, Feingold shows himself an energetic researcher, employing all of the tools of the historian to document these and other conclusions in assiduous—one is tempted to say tedious—detail. He makes his case, without doubt, and in so doing provides much new information that causes one to ponder, and reevaluate, a number of generally accepted “truisms” about the Scientific Revolution. If his work has any defect, it is that he gives us too much information, too much detail, obscuring the larger picture he is trying to sketch. But subsequent work is substantiating many of his insights, showing that the much-maligned Aristotelian science of the universities was never inimical to mathematics and indeed, in England as on the Continent, provided the seedbed from which the new science emerged later in the seventeenth century.

WILLIAM A. WALLACE
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L. M. HILL. *Bench and Bureaucracy: The Public Career of Sir Julius Caesar, 1580–1636*. Stanford: Stanford University Press. 1988. Pp. xv, 316. \$37.50.

Based on L. M. Hill's 1968 Ph.D. thesis, this study of the public career of Sir Julius Caesar has been a long time coming. It is well worth the wait.

Born in 1558, the son of an Italian immigrant who became a fashionable court physician, Caesar was only ten when his father died. Characteristically Caesar turned that setback to his advantage by using both his stepfather's relatives and Lord Burghley, who held his wardship, to gain entry to Oxford, eventually becoming a doctor of civil law. In 1587 Caesar was made an admiralty judge. He used his office to modernize the court in order to handle the increased business produced by the naval war with Spain, while protecting the court's jurisdiction from the encroachments of the common lawyers. When, four years later, he also became master of requests, Caesar was just as assiduous in protecting and reforming that second prerogative court.

As chancellor of the exchequer from 1606 to 1614, Caesar tried hard to deal with the rapidly growing budget deficit, which he told Lord Treasurer Salisbury “cannot last, but will bring a dearth.” The pair tried to resolve that familiar problem with the proposed Great Contract of 1610, whereby the king would surrender unpopular revenues based on the royal prerogative in return for a increased annual parliamentary grant. But at the last moment negotiations between the crown and Parliament collapsed, in part

because of the memorandum that Caesar (who as a civil lawyer was most jealous for the royal prerogative) wrote opposing the agreement.

From 1614 to 1636 Caesar served as master of the rolls, the most senior active judge in chancery. The post was the apex of a civil lawyer's career. As usual he attempted reform, while resisting all who trespassed on his official patronage and profits. He died in harness at the ripe old age of seventy-eight, leaving behind a reputation for hard work, honesty, and ambition; a happy and prosperous family; and a gold mine of papers, which Hill has worked to produce this valuable monograph.

Caesar was of the second rank of crown servants; he was one of those men who made the system work. His ability to use several patrons at the same time in his career belies the notion that inclusive factions dominated early modern politics. The emphasis in Caesar's public life on precedent rather than on political theory reminds us of the essentially conservative nature of early modern government.

CHARLES CARLTON
North Carolina State University

PETER LAKE. *Anglicans and Puritans? Presbyterianism and English Conformist Thought from Whitgift to Hooker*. Boston: Unwin Hyman. 1988. Pp. 262. \$49.95.

Peter Lake has continued his explorations of Elizabethan Protestantism with this superb analysis of the debates between Presbyterians and mainstream Anglicans (or “Conformists,” as Lake would have it). This study could be called a younger sibling of Lake's *Moderate Puritans and the Elizabethan Church* (1982), for it covers much of the same period and some of the same participants. The focus, however, is sufficiently different so that this study stands as its own important contribution to the renewed discussions of Elizabethan and early Stuart Protestantism. Lake, along with historians such as P. Collinson, N. Tyacke, J. P. Sommerville, and, from a different point of view, P. Christianson, has offered a complex and lively picture of English Protestantism.

Lake is here concerned with examining the agreements and disagreements within Elizabethan Protestantism. He recognizes that in many areas and especially in their methods and their underlying assumptions the Elizabethan Protestants shared much, but he also knows that there were significant differences. The differences were serious to the participants and their contemporaries and, therefore, merit the historian's serious consideration.

Lake's serious consideration of differences

within Elizabethan Protestantism develops around the issues of Presbyterianism and the responses Presbyterianism evoked. His examination of the Admonition Controversy is presented as a debate between Thomas Cartwright and John Whitgift, whose shared Calvinism could not cover their vicious disagreements on the visible and invisible church, the role of clergy, and the church and state. Whitgift appears to have offered the more generous definition of Christian, but he was the more restrictive in defining participation in civil government.

Others followed Cartwright and Whitgift in extending the debate for two decades and enlarging its issues. The moderate Presbyterianism of Walter Travers and Dudley Fenner in the early 1580s projected a lingering hope for official reform of the Church of England, but, by the end of the decade, the more radical Presbyterianism of John Penry, John Udal, and the Marprelate tracts reflected realistic disappointment in official reform. On the other side, Whitgift's argument that the Christian prince may choose an appropriate ecclesiastical organization became a moderate stance by comparison to the exclusive *jure divino* episcopacy that Richard Bancroft and others urged in the early 1590s.

Lake's impressive analysis of the range of arguments within the Elizabethan church serves as a prelude to his expansive presentation of Richard Hooker's views. Lake contends that Hooker, although appearing to be within the tradition of anti-Presbyterianism, used that tradition as the foundation for a platform from which to attack the larger enemy—Calvinist pietism. Against Calvinist pietism Hooker offered a community of worshippers who were bound and instructed by prayers and sacraments. Such views formed a non-Arminian alternative to Puritanism, for, as Lake concludes, "Hooker undoubtedly deserves his place in 'anglican' hagiography—not because he personified or expressed existing 'anglican' attitudes and values but because he, more than anyone, invented them" (p. 230).

If there is some sense that this book could be described as "readings" of Cartwright, Whitgift, Hooker, and others, it is because the author wisely refers to rather than rehearses the larger political and social context about which other historians have written. He has instead concentrated on thoughtful and provocative analyses. Lake has written another important study, but this publisher unhappily has offered it in a difficult typeface and a poor binding. Lake and his readers deserve better.

MARVIN A. BRESLOW
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HUGH TREVOR-ROPER. *Catholics, Anglicans, and Puritans: Seventeenth Century Essays*. Chicago: University of Chicago Press. 1987. Pp. xiii, 317. \$27.50.

The five essays in this volume constitute a major contribution to the intellectual history of England during the first six decades of the seventeenth century. Two of them, however, that on Nicholas Hill the atomist and that on John Milton's political career, are marginal to the central focus of the volume and, in this limited space, have to be disregarded. The essay on Hill, the shadowy late Elizabethan philosopher of nature, is stimulating, wide-ranging, but ultimately speculative, and that on Milton is not especially original.

Central to the volume are the three essays on Laudianism as a political movement, on the Great Tew circle that began in the 1640s around Lord Falkland, and on Archbishop Ussher. In the course of these three essays, Hugh Trevor-Roper presents a coherent account of the intellectual, political, and religious origins and the character and consequences of what he persists in labeling "the Puritan Revolution."

The author's central argument is that Laudianism, which played such a major part in provoking the political crisis of 1640, must be distinguished from Arminianism, "an intellectual movement which, in England, went back to the time of Erasmus" (p. 114) and which had long "appealed to humane and liberal men" (p. ix). Arminianism flourished in England even before the Dutch theologian Arminius was born and was a significant part of the Elizabethan and Jacobean Protestant consensus. Only when, under Richard Bancroft, it became allied with "episcopalian clericalism" and when, under Charles I and William Laud, it became more fatally linked with "royal absolutism" and became Laudianism did there develop "the new Caroline synthesis of religion and politics which alarmed the political classes and drove them, ultimately, to revolt" (p. 114).

While the Laudians were enjoying the fruits of political office, a loose association of liberal and humane scholars who also fed on the Erasmian and Arminian tradition came together at Lord Falkland's house in Oxfordshire. This group, however, was, in Trevor-Roper's opinion, more faithful to the original rational skepticism of Erasmus and his followers. The history of this group through the 1630s is well known. What the author adds to the familiar story is the role played by members of this group during and after the Civil War. The Great Tew circle was broken in 1641 but not shattered. Survivors such as Edward Hyde, Henry Hammond, and George Morley "remained remarkably united," and in 1660 "they achieved their triumph (when) Crown and Church were

restored on something like the terms they advocated in 1641" (p. 228).

Complementing this account of the Laudians and of the Great Tew group is an illuminating essay on James Ussher, in which Trevor-Roper rejects the traditional view of the Irish archbishop as an admirable moderate trying to achieve a compromise among men of extreme views and sees him, instead, as a man who, by 1641, was an anachronism. When the Long Parliament met, Ussher was too old to break away from his late Elizabethan assumptions that combined "apocalyptic theory, providential history and underground continuity" in an amalgam that was "out of date" (p. 152).

Like all of Trevor-Roper's historical writings, this volume is rich, stimulating, and plausible and runs counter to the natural tendency of English historians toward insularity. One of Trevor-Roper's great strengths is his ability to place Laud and his contemporaries in their European context where the author is more at home than the vast majority of English historians writing at the present time. At the same time, there is something anachronistic about his view of seventeenth-century English politics and religion.

Like Ussher, Trevor-Roper is imprisoned by assumptions that are less credible than they once were. He takes for granted that the central political event in seventeenth-century England was a "Puritan Revolution" and is untroubled by the assumptions underlying that term. Notwithstanding his strictures on the notion that "the whole concept of linear progress as the distinguishing property of certain political parties" is "unhistorical" (p. xiii), his whole approach to this historical period is linear. It is clear that he admires and identifies with the humane and liberal tradition of Erasmus, Grotius, and Clarendon. It is also clear that he finds Catholicism unacceptable because of its illiberalism. Referring to William Chillingworth's brief experiences of the Catholic church, he comments that "he there discovered, as others have discovered since, that the Catholic Church, which can look so liberal to the potential convert, shows a very different face once he is inside" (p. 203). Trevor-Roper's values, which are very much those of his own time and place, provide an essential dimension of his narrative and make his heroes admirable, recognizably modern, but of questionable authenticity.

MICHAEL G. FINLAYSON
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KEVIN SHARPE. *Criticism and Compliment: The Politics of Literature in the England of Charles I.* (Cambridge Studies in Early Modern British History.) New

York: Cambridge University Press. 1987. Pp. xiii, 309. \$49.50.

This "book about literature by a historian" (p. 1) is an illuminating study of Caroline court culture that provocatively challenges received ideas about the relationship of culture to politics in early Stuart England. Kevin Sharpe begins by criticizing the view that distinct and antagonistic court and country cultures emerged in this period. He shows that the connotations of the terms "court" and "country" were more complex than have usually been recognized and argues that they stood for complementary, rather than opposing, ideals. He also points out that the court was often criticized by its own members, many of whom shared attitudes modern historians have associated with a country ideology.

Sharpe sees Caroline court literature as the product of a "humanist" tradition that stressed the poet's role as a counselor who had to offer both praise and criticism to the prince. The poets fulfilled that role, he contends, by using the theme of love as "the metaphor, the medium, through which political comment and criticism were articulated" (p. 39). He develops that thesis through a close study of three relatively neglected poets who each wrote one or more court masques: William Davenant, Thomas Carew, and Aurelian Townshend. Sharpe's discussion is continually enriched by his impressive grasp of court politics, especially his awareness of dissension and factionalism, which enables him to show that the political messages conveyed by court literature were richer, more ambiguous, and more interesting than previous scholars have suspected. His discussion of "cavalier" drama, in which he takes issue with Alfred Harbage and Martin Butler, is particularly useful, and his sophisticated and intelligent discussion of the Caroline masques is probably the best essay yet produced on the topic.

There are, almost inevitably in a book as ambitious and provocative as this, a few arguments that do not convince. Sharpe's characterization of the philosophical underpinnings of court culture is oversimplified. He convincingly documents a certain tension running through court literature, between an austere Neoplatonic philosophy that denied the value of all forms of sensuality and a more moderate and balanced Aristotelian attitude that sought to reconcile sense and intellect. But he ignores other traditions that influenced the court's outlook, including classical and Renaissance historiography, Stoicism and Neo-Stoicism, Epicurean motifs, and Erasmian strains of Christianity. Taking those into account might not have required any fundamental changes in the book's argument

but would have rendered the analysis more complex and subtle.

A second troublesome feature of this book is its persistent effort to associate Charles I with the ideal of Platonic love, which Sharpe sees as a metaphor for intransigent and autocratic political attitudes. Platonic love is an ambiguous term that can refer to a cult of sexual abstinence that may have flourished around Queen Henrietta Maria, to an austere Neoplatonic doctrine that rejected human sensuality, or to more flexible efforts to purify and spiritualize sexual love without denying its physical basis. Sharpe explains all of this and acknowledges that the evidence for the existence of a Platonic love cult at court and for the king's relationship to it is slight (pp. 22–23). Later in the book, however, those distinctions and cautions are left behind, and Platonic love is described, without qualification or definition, as a preoccupation of both king and queen (p. 65) and as a metaphor for the court's "political ideology" (p. 139). Sharpe provides little support for these views apart from the assertion that "scholars have identified" Neoplatonism and the cult of Platonic love as the central philosophy of the masques, plays, and poems of Charles's court (pp. 22–23). His own analysis of Caroline literature demonstrates the inadequacy of this view.

Such criticisms should not be allowed to obscure the fundamental importance and originality of Sharpe's book, which is usually persuasive and always provocative and interesting. It is also written with enviable boldness and clarity. No student of Stuart literature or politics can afford to neglect it.

R. MALCOLM SMUTS
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JAMES ANDERSON WINN. *John Dryden and His World*. New Haven: Yale University Press. 1987. Pp. xviii, 651. \$29.95.

Because of its emphasis on the roles of politics and finances in John Dryden's life, James Anderson Winn's book is more than a standard literary biography. He clearly demonstrates how a knowledge of the times in which Dryden wrote is essential to understanding why he wrote what he did and why he wrote the way he did. By showing how his plays, poems, satires, and other works filled contemporaries' needs and desires, Winn situates them in the world where they first appeared and, more important, opens up that world to us today.

The book is handsomely produced with many well-chosen and appropriately placed portraits. The notes, essential to understanding the main

text, are unfortunately placed at the back. The lack of a bibliography is annoying.

Winn's sophisticated and informed discussions of intellectual topics such as the diversity of ideas among Puritans and the importance of family ties are valuable. Basing his interpretations of national and international events mostly on a limited selection of works by historians such as David Ogg and J. R. Jones leads to some oversimplifications; I doubt, for example, that Louis XIV had only to choose to invade England in 1690 to be successful.

Winn accepts at face value Dryden's complaints about being poor; without quantitative data that simply do not exist, it is impossible to know whether he really was frequently in financial straits. Anecdotal data (providing an expensive education for his sons, the apparent lack of harassment by creditors, and purchasing expensive watches in 1696) suggest a reasonably secure financial situation.

Although Winn frequently draws on his understanding of Dryden's life as a whole when evaluating literary works, he does not do so when dealing with personal aspects of the poet's life. For example, was Dryden's lifelong unwillingness to become a Protestant clergyman a result of early Catholic "tendencies"? Did Dryden hope to benefit financially from becoming a Catholic and were the overdue payments he received from the treasury soon after his conversion a reward? When Thomas Shadwell replaced Dryden as poet laureate after the Glorious Revolution, he used the office to hurt "Dryden whenever he could" (p. 460). Did Dryden use the office for similar purposes before 1688? Personal questions like these are usually not addressed in this biography, perhaps because they do not lend themselves to discussion in a literary context.

Winn achieves his goal of providing "a fuller sense of the man and his times" (p. xiii) than previous biographers of Dryden have done, but something still seems missing. Winn makes many imaginative and even daring literary interpretations, but he has not re-created a living, breathing, feeling Dryden—a flesh-and-blood man with emotions and a daily life, a man who was perhaps even nasty and vicious at times. This omission reflects the paucity of nonliterary primary sources about Dryden. Nevertheless, since it is unlikely that anyone with Winn's knowledge and feel for Dryden will appear again soon, it is disappointing that he did not use his imaginative understanding to flesh out the nonliterary aspects of Dryden's life more fully.

WILLIAM ROOSEN
Northern Arizona University

CRAIG W. HORLE. *The Quakers and the English Legal System, 1660–1688*. Philadelphia: University of Pennsylvania Press. 1988. Pp. xv, 320. \$34.95.

Craig W. Horle's book, a welcome addition to the literature of the great persecution of Dissenters, documents the confrontations between Quakers and agents of law enforcement in the Restoration period. Working with several groups of manuscript records of "sufferings" and correspondence from the library of the Religious Society of Friends (London), complemented by records of quarter sessions from a dozen counties, Horle has compiled a detailed description of charges brought against Quakers and how they dealt with them. The story that emerges is one of a sect that grew in numbers from two hundred fifty in the early 1650s to forty thousand by 1660 and reached a peak in 1680 (p. 15, no numbers given), with at least three hundred deaths attributed to persecution between 1660 and 1688. This growing membership was supported and encouraged by a powerful organization that after 1675 directed the network of monthly and yearly meetings from London headquarters and urged members of the sect to keep detailed records of persecution and harassment, to complain in writing to authorities, and to enlist the services of defense counsel. George Fox, the inspirational leader and the principal organizer of the sect (until his death in 1691), is credited with implementing this national system after his release from a two-year imprisonment.

The book opens with a general introduction explaining which Quaker beliefs and practices were either contrary to law or offensive to authorities and neighbors. Chapter 1 outlines in admirable detail the complex laws and procedures under which Quakers and other Dissenters could be prosecuted in local, central, and church courts. The vacillating attitudes and policies of Charles II and James II and their parliaments toward Quakers is traced in chapter 2, and Horle provides background information with details of the influences and incidents that affected the varying degrees of persecution at different times during the three decades. Chapter 3, "The Tyranny of the Law," takes up the Quakers' clashes with the law, describing mass arrests and inhumane imprisonments—illustrated with detailed case studies, activities of informers, and brutal (and often illegal) conduct of some overzealous law-enforcement officials.

The next chapters, "The Lamb's War" and "Quaker Legal Defense," provide the answers to the questions posed by the author at the outset (p. 17): "How were the Quakers able to survive, and why did the Anglican Church and the English government fail to eradicate them?" The impres-

sive organizational skills of the London leadership, assisted by professional counsel (listed in appendix 2), and the fortitude of Quaker victims themselves, aided by sympathetic neighbors and officials and by the influence Quaker leaders could, on occasion, command in royal circles, all contributed to the survival of the sect. The book concludes with a summary of other contributing factors, including the cumbersome ineptitude of the English legal system, the lack of consistency in enforcing the laws against religious dissidence, and the fluctuating positions of both crown and parliament. Horle's evidence tells how many of the English, from neighbors and constables straight up the social ladder to assize judges and bishops, had no stomach for enforcing the statutes of religious conformity that had accumulated since the early sixteenth century. The historical record Horle has mined tells a tale of cruelty and harassment on the part of many that was counterbalanced by others more sympathetic and humane who did much to alleviate the harshness of the law. The book is well focused, and Horle fulfills what he set out to do. I question whether the "collective set of principles" developed by Quakers by 1660 "threatened the foundation of the English legal system" (p. 16), and a few slips detract (surely Quakers cannot be held responsible for supporting "the overthrow of the monarchy and the church" in 1649 [p. 271]). But these are minor flaws in this carefully crafted work that is a valuable and substantive contribution to the literature of the early Quakers and, perhaps more important, to our knowledge of the relation between Restoration law enforcement and politics.

NANCY L. MATTHEWS

Smithsonian Institution Libraries

JOHN BENDER. *Imagining the Penitentiary: Fiction and the Architecture of Mind in Eighteenth-Century England*. Chicago: University of Chicago Press. 1987. Pp. xvii, 337. \$29.95.

Historians are perhaps more accustomed to thinking of art or literature as displaying the sensibilities of an age rather than as creating them; reading John Bender's innovative study, with its abundance of well-chosen illustrations, should stimulate them to consider a different viewpoint. He argues that narrative literature and art shaped society's attitudes toward the English penal system before 1779 so that they "enabled" the conception and later construction of the penitentiary. The penitentiary idea, with its emphasis on individual reformation through the combined impact of psychological and material structures, is brilliantly

captured by the book's theme of "the architecture of mind."

The historical origins of the Penitentiary Act of 1779, which proposed the erection of the first two penal institutions, have hitherto not been explained very satisfactorily. Michael Ignatieff suggested in *A Just Measure of Pain* (1978) that this legislation was the product of two essentially short-term causes: John Howard's *The State of the Prisons* (which two years previously had revealed appalling penal conditions) and the practical crisis precipitated by the American war of the 1770s, with the ensuing attempt to accommodate prisoners, rather than transport them to the colonies. And Sean McConville, in *A History of English Prison Administration, 1750–1877* (1981), noted the lack of sustained public concern for the state of the prisons during most of the eighteenth century and rationalized this with the suggestion that their unreformed state was seen to be accepted precisely because it maximized the deterrent purpose of the penal system. Neither hypothesis adequately fits the historical evidence, where a pattern of repeated concern was discernible in parliamentary inquiries before 1779.

This attempt to elucidate the ideological prehistory of the penitentiary is therefore very welcome, and Bender notes that his study ends where conventional accounts of the penitentiary movement begin. In a detailed analysis of selected Georgian writers and artists, Bender argues convincingly that they provided a reform discourse that stimulated prison changes through shaping contemporary consciousness. Daniel Defoe, viewed by his peers as a specialist in prison narrative, revealed the possibilities of transformation in a place of confinement; John Gay exposed the contradictions present in the old regime; William Hogarth's visual narratives rejected the old prison and implied an alternative; and Henry Fielding's convergent interests as magistrate and novelist led him to congruent innovation in both spheres.

Bender's insight—that the penitentiary idea was a reform construct aimed initially at the literate and only later addressed to an audience of criminals in architectural form—is a persuasive one. So, too, is his view that Jeremy Bentham's reforming ideas were influenced by this earlier novelistic literature. Less convincing is his view of the character of early penal establishments, since it ignores recent historical studies—notably by Joanna Innes—which have questioned the penitentiaries' stereotyping as inefficient. And I also remain skeptical about the extent of the alleged reciprocity between institution and cultural context, where Bender contends that the change from the randomness of the unreformed prison to the structure of the reformed institution corresponded to

related changes in literature, when narrative order emerged in realistic fiction.

ANNE DIGBY
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Oxford University

JULIAN HOPBIT. *Risk and Failure in English Business, 1700–1800*. New York: Cambridge University Press. 1987. Pp. vii, 228. \$39.50.

T. S. Ashton once wrote about a wave of gadgets that swept over England after 1760, inventions that were inextricably related to the preeminent names of the Industrial Revolution, Matthew Boulton and James Watt, Richard Arkwright, Robert Peel, and Josiah Wedgwood. It is all too easy to forget that these heroic figures are known because they succeeded and that hundreds or even thousands of other would-be entrepreneurs, whether inventors, innovators, or imitators, were not so lucky. As Julian Hoppit puts it, "Many businessmen and many potential businessmen were simply in the wrong place at the wrong time, doing the wrong things and with inadequate resources or skills at their disposal to see and then seize the sorts of opportunities which were being created and made available" (p. 14). For these men there were to be no historical garlands, but from bankruptcy records Hoppit has attempted to rescue some of the thirty-three thousand would-be Wedgwoods and Foleys who failed and who thereafter had to live not merely with the stigma of bankruptcy but also with the legal disadvantages that accompanied it.

Hoppit uses a combination of bankruptcy declarations in the *London Gazette* and the so-called docket books in the Public Record Office to track down the course and incidence of bankruptcy. What he finds is, at face value, rather surprising: the English Industrial Revolution was accompanied by a rise in the number of bankruptcies. In real terms a tripling of the incidence of bankruptcy seems likely through the eighteenth century with an even higher increase occurring during the final decades. Men and women were alive to the degree of opportunity but failed to judge the degree of risk as decision making took place in an increasingly unpredictable environment. Entrepreneurs in the textile and food and drink trades and wholesalers were the most prone to bankruptcy, and they were most likely to be found in the southeast—especially in London—or in Lancashire and Yorkshire. They failed for all sorts of reasons, among them the vagaries of the trade cycle, the destabilizing effects on the economy of war, and the unpredictability of financial crises.

This is an important book because it makes a

substantial contribution to our knowledge of business and the Industrial Revolution, but it also raises questions to which Hoppit has no real answers. Most critically, will the bankruptcy records stand as a proxy for the level of business failure in eighteenth-century England? The law covered only certain types of insolvency, so that Hoppit's statistics are not necessarily an accurate reflection of the volume of failure. An increase in bankruptcies may point to nothing more than an increase in the size of businesses as the Industrial Revolution proceeded. Put in another way, bankruptcies may have been common in Lancashire and Yorkshire because the scale of textile businesses was greater than, for example, the scale found in the hosiery industry in the East Midlands. Why, otherwise, were Derbyshire, Nottinghamshire, and Leicestershire among the counties with the least number of bankruptcies? Hoppit is aware that these are problems that raise questions about the use of his statistics to indicate trends in the eighteenth-century economy. The consequence, however, is that, although the book tells us much that is new and interesting about bankruptcy (although sadly all too little about individual cases), it may not tell us as much about the English economy at the outset of the Industrial Revolution as Hoppit hopes.

J. V. BECKETT
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WILLIAM STAFFORD. *Socialism, Radicalism, and Nostalgia: Social Criticism in Britain, 1775–1830*. New York: Cambridge University Press. 1987. Pp. viii, 304. Cloth \$42.50, paper \$15.95.

William Stafford's readable book contains a multitude of interesting observations on early nineteenth-century England together with brief summary discussions of ten critical tracts written between 1775 and 1830 by Thomas Spence, William Ogilvie, William Godwin, Charles Hall, S. T. Coleridge, Robert Owen, Percy Ravenstone (Richard Puller), William Thompson, Thomas Hodgskin, and William Cobbett. Stafford gives no particular reason for his choice of texts (he omits important propagandists such as John Horne Tooke, Major John Cartwright, Richard Price, and Thomas Paine), and he remarks that the only thing his materials definitely have in common is the fact that they all contain "critiques of society" (p. 6).

What then is Stafford's purpose? Is it to provide, as his title promises, some coherent interpretation of the social ideas of the period? Or is it merely to supply exegeses of ten dissimilar texts, relating them to each other only by a few tentative gener-

alizations? Unfortunately, the latter seems to be too much the case. The book's main deficiency is its weakness of interpretation. Only seldom as he moves through his ten texts does Stafford relate one to another, and in fact he never clarifies the meaning nor attempts to explain the general application of the terms in his title. His third chapter on "Mental Furniture" promises an account of the main ideas gleaned from his ten texts, but it disappointingly contains only a laundry list of all of the ideas that might have influenced his authors, a list including virtually every conceivable "intellectual component" but mesmerism and phrenology. It is as though, having been promised a discourse on, say, the principles of aviation, we were shown instead a huge bin of airplane parts.

There are signs that behind Stafford's interpretive timidity stands a deficiency of reading in the primary literature of the period 1760–89. Stafford begins his exegeses by summarizing Spence's demands in 1775 for nationalization of land ownership and then remarking (p. 102): "Spence's plan was formulated fifteen years before the shock of the French Revolution. Here is an absorbing historical problem. What made Spence's plan possible?" Given a better acquaintance with contemporary literature, he might have seen immediately that Spence's text represents a logical extension into the sphere of land ownership of the Norman Yoke theory so much employed by radicals in the 1760s, and, further, that Ogilvie, Hall, Thompson, Hodgskin, and Cobbett all worked later from the same intellectual base. Beginning with Spence, Stafford might thus have evolved a general interpretation, but instead he contents himself with reducing Spence's text to biographical details and the prefabricated gridwork of "mental furniture." It is not until the conclusion, four pages from the end of the book, that Stafford clearly states the notion that "a developing critique of exploitation" can be traced from Spence down through many of his successors, a critique that "begins as an offshoot of 'the Norman yoke' myth," expands to include capitalists among the class of exploitative conquerors, and then "can justly be regarded as leading on to Marx" (p. 270). If he had worked from the beginning with a handful of substantial ideas like this one, Stafford might have produced something interpretively more interesting. His individual commentaries contain many sensible observations and are well worth reading, but for overall guidance we must still look to the classic writings of such historians as Élie Halévy, G. D. H. Cole, and E. P. Thompson.

GERALD NEWMAN
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J. B. CONACHER. *Britain and the Crimea, 1855–56: Problems of War and Peace*. New York: St. Martin's. 1987. Pp. xi, 289. \$39.95.

The collapse of Lord Aberdeen's government in January 1855 brought about a crisis in British politics that led to the reformation of the coalition of parties into a ministry headed by Lord Palmerston. That ministry and its conduct of the war in the Crimea are the subjects of this volume, which can be seen as a sequel of sorts to J. B. Conacher's acclaimed earlier work, *The Aberdeen Coalition, 1852–1855* (1968). Together the two volumes constitute the most thorough and modern interpretation of this crucial period of British politics and diplomacy.

Conacher discusses the events of the last two years of "this strange war" from two perspectives (p. 162). On the military side he makes clear that the poor quality of leadership in the field and the lack of imagination of the commanders in tactical and strategic terms highlight the almost accidental nature of the allied victory after Sebastapol. The generals were not entirely to blame, however. The secretary of war, Lord Panmure, was at best a mediocre figure, and the cabinet as a whole failed to strengthen the army in the field. As a result the army, even including auxiliaries, was never more than half the size of the French forces. The government's failure to support the armed forces is the "greatest indictment" against the Palmerston coalition's military policy (p. 110).

Conacher's other perspective focuses on events in diplomacy that we might see as a drift toward peace rather than as a policy more deliberately pursued. The ministers in London mistrusted, with cause, everyone in the French government except Louis Napoleon, with whom they dealt personally as often as possible through Lord Cowley, the ambassador in Paris.

Austrian mediation and the ultimatum of early 1856 helped bring the parties to the peace convention in Paris. Of those assembled, the British were clearly the most reluctant participants, as they had some cause to hope for greater victories in the anticipated campaigns. But, as war is fraught with risks, and given the quality of the military leadership and the uncertainty of French support, the pragmatic natures of both Palmerston and the earl of Clarendon, the foreign secretary, ultimately enabled them to strike a compromise that concluded the war in ways deemed honorable at home and abroad. Conacher's principal conclusion, in fact, is that the peace was "not the diplomatic failure that some historians have suggested" (p. 221).

One of the more interesting aspects of this thorough and highly detailed book is its insights

into the interplay of personalities in politics. Conacher has made extensive use of the papers of all of the principal political figures. His study clearly enhances Clarendon's reputation as a statesman and man of ability and portrays Lord Derby and Benjamin Disraeli as too often inclined to take contradictory positions for the sake of party advantage. The sensibility of the court, especially of the young queen, is also noteworthy. Finally, Lord Russell remains as we know him to have been, a man of great talent but erratic.

One final theme informs Conacher's scholarship, but restrictions on space required him to eliminate all but the conclusions of his enquiry into the relationships between the Peelites and the Liberals as they were influenced by the events of these years. The full integration of the Peelites into the Liberal party was certainly not complete by the spring of 1856. As late as the end of the war, William Gladstone was willing to return to the Conservative benches, though not alone, if for no other reason than to assure the efficient working of Parliament through the presence of a two-party, as opposed to a multiparty, arrangement. That and other comments about the important theme of party entice us to anticipate another volume from this distinguished historian.

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JAMES E. BRADLEY. *Popular Politics and the American Revolution in England; Petitions, the Crown, and Public Opinion*. Macon, Ga.: Mercer University Press. 1986. Pp. xii, 264.

James E. Bradley has added new information to a subject much studied over the last generation. He has pursued every available petition to Parliament and address to the crown, particularly those in the first years of the crisis that grew into the American revolution. The government, Bradley insists, took such expressions of opinion seriously, sought to minimize the impact of petitions in Parliament, and took countermeasures by organizing loyal petitions and addresses. The pivotal year, for Bradley's purposes, was 1775. America had not yet formally chosen independence, and the fighting had not yet escalated into another Anglo-French war. The issue being tested was approval or disapproval of the government's policy of coercion in America. Opinion, as represented in petitions, pamphlets, and press, encompassed a range of positions. Some disliked force and prayed for negotiation. Others proposed greater or lesser concessions. A very few took a strongly pro-American position.

Ideological differences, Bradley argues, divided the kingdom. Previous studies, in his opinion, have underestimated that rift. What was at stake, he suggests, was high political principle, not the religious or economic differences cited by most scholars. Bradley then tests "political consistency" by comparing signatures on petitions, subscription lists, and voting behavior in contested elections. He examines five communities—Great Yarmouth, Cambridge, Southampton, Bridgwater, and Poole—in depth to elicit "the inner motivation of the petitioners themselves" (p. 174).

Herbert Butterfield argued in *George III, Lord North and the People, 1779–1780* (1949) that a quasi-revolutionary crisis developed and was narrowly averted in England when the anti-Catholic Gordon riots of June 1780 discredited notions of democratization. Butterfield's thesis, Bradley believes, should not be casually dismissed. A storm was brewing in English politics, and the American crisis and petitioning campaign of 1775, the scope of which, he argues, has also been underestimated, was a symptom. Some of this is new; some is not. Both the Wilkite agitation that preceded the American revolution and the Association movement that evolved after the colonial war expanded into war with France have been much studied. So has pro-American sentiment both inside and outside Parliament. In his pioneering studies of popular politics in late eighteenth-century England, John A. Phillips refined the technique that Bradley employs of comparing signatures on petitions and voting behavior in poll books.

Bradley originally sought to explore "the political influence of religion in a revolutionary setting" (p. ix). He grafts his considerable understanding of Protestant Dissent in local and national politics onto Phillips's method of studying politics and opinion. Bradley also uses the provincial press more rigorously than any previous student. Understanding what newspapers say, however, does not necessarily tell us what public opinion was. Some specialists, moreover, would argue that the pamphlet or broadsheet was still the medium of choice for influencing opinion.

Both sides, Bradley argues, sought to convey an impression of support from the public "out of doors." The government, he finds, used the *London Gazette* effectively to suggest a nation firmly behind the policy of repressing the Americans. Court loyalists and American sympathizers mobilized meetings and procured petitions and addresses as political weapons. Lord North used his parliamentary skills and an overwhelming majority to minimize the impact of pro-American petitions while the government and its supporters simultaneously promoted widely publicized loyal addresses.

Bradley takes issue with Ian Christie and others who have argued that English radicalism had a local rather than a national basis. Yet Bradley himself actually contributes some weighty evidence to that view. He suggests that the same divisions continued even when America was no longer the principal subject in dispute. So, too, he points to other earlier bases for controversy: Protestant Nonconformists who sought relief from the provisions of the Test and Corporation acts and Dissenters and their supporters who sought repeal of the provisions of those statutes that legally impaired, even technically prohibited, their full participation in political life. Petitioners often used the issues at hand to further highly particular agendas. When Bradley asks "why hundreds of Dissenters and thousands of Anglicans all across the nation responded to the American crisis at the same instant" (p. 202), the answer may simply be that it was convenient and timely. Polling past public opinion is a difficult task. Our modern sense of public opinion is, in many respects, ill suited to the eighteenth century. England was not united on a policy of coercion, and Bradley is misleading when he implies that to be the view of many specialists. Bradley initially announces, "The government's majorities in the early years of the war represented the opinion of a small minority of the English public" (p. 89), but even he retreats to a more judicious view by chapter 7. Bradley suggests the quinquennial flurries of petitioning movements between 1769 and 1783 show a parallel pattern of popular and electoral politics (p. 121). But contested elections are by no means a satisfactory index to the state of opinion, nor do they necessarily reflect actual divisions within constituencies. People sometimes signed petitions, as Dr. Johnson pointedly remarked in *The False Alarm* (1770), for a variety of reasons that had little to do with their content.

Bradley's rigorous research commands respect, although its presentation leaves something to be desired. Bradley is ill served by his publisher, whose overinked pages sometimes render the text all but unreadable. Some editorial carelessness does not help. Short title footnotes occasionally appear before the full citation is given. Bradley marshals his many tables well and explains them clearly, although at times the reader may wonder if they are all necessary. The index is useful, although the reader must sometimes be imaginative. Sedition, for instance, is an important issue but is to be found buried under "Petitions to the Crown: and Bill of Rights."

This is a useful book, crammed with information, but not easy to read. Bradley is often repetitive. His points, although clearly developed, seem labored. Bradley does carve out useful new

ground, but much of what he covers is familiar. His most interesting findings concern the five towns he analyzes in depth. Bradley's documentation is formidable but does not always demonstrate the points he attempts to make. He has, however, important things to say about the workings of Dissent in English political life.

EUGENE CHARLTON BLACK
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T. A. JENKINS. *Gladstone, Whiggery, and the Liberal Party, 1874–1886*. New York: Clarendon Press of Oxford University Press. 1988. Pp. vi, 328. \$69.00.

The Whigs have been on a roll of late. Recent important books by Richard Brent, *Liberal Anglican Politics: Whiggery, Religion, and Reform 1830–1841* (1987), and J. P. Parry, *Democracy and Religion: Gladstone and the Liberal Party, 1867–1875* (1986), have effectively argued that an appreciation of the seriousness of Whig religious purpose is crucial to a full understanding of nineteenth-century Whig politics. Parry explained the Liberal defeat in the general election of 1874 as the result primarily of Whig-Anglican rather than Radical-Nonconformist disaffection, and he found in the religious divisions afflicting the Liberal party during the early 1870s a prefiguration of the later split over Irish Home Rule. T. A. Jenkins does not have much to say about Whig religion, but he does forcefully maintain that the strength and tenacity of Whig influence within the party in the decade after 1874 have been consistently underestimated.

The historiographical context for Jenkins's impressive analysis is the debate over the break-up of the party in 1886. Was Gladstone's initiative on Irish Home Rule the cause of the break-up or merely its occasion? Was the divide between Whigs and Radicals so deep by 1885 that unity could not be sustained whatever was done or not done about Ireland? In Jenkins's view, the split was by no means inevitable. Before the end of the 1870s, Hartington had proved himself an able leader in the House of Commons acceptable to the bulk of the party. Had it not been for the probably unwarranted connection made by many between Gladstone's spectacular Midlothian campaign and the unexpectedly large Liberal victory of 1880, together with Gladstone's refusal to serve in a ministry of which he was not the head, continued Whig dominance of the party at the top could not have been successfully challenged. From 1880 to 1885, Joseph Chamberlain's presence in the cabinet notwithstanding, the materials for a remaking of the Liberal party in Joe's image were conspicuous by their absence. There was tension between Whigs and Radicals, a tension heightened by the

uncertainty generated by the electoral reform package of 1884–85, but neither side considered a purge of the other as sensible or feasible.

The upshot is that responsibility for the schism lies with Gladstone alone. "Gladstone . . . by prematurely forcing the issue of Irish home rule and staking his own political career upon it, succeeded in creating an artificial division among the Liberals, and enabled the Conservatives, with their Liberal Unionist allies, to establish themselves as the 'natural' majority party" (p. 292). Although Jenkins is prepared to concede that a complex psychological process allowed Gladstone to infuse what was in essence a politically self-serving conversion with genuine moral conviction and significance, he ultimately charges Gladstone with a "failure of leadership" (p. 292) that did great and unnecessary harm to the Liberal party.

Inasmuch as the Whigs are given center stage in this excellent monograph, Gladstone is a somewhat shadowy figure throughout most of its pages. Nonetheless, it emerges clearly that Gladstone occupied a preeminent place in the consciousness of Whigs and Radicals alike. According to Jenkins, most of those who did not defect in 1886 stayed with Gladstone not because of any enthusiasm for Home Rule but because of a profound faith in its illustrious sponsor. Those who seceded did so in the main not because of any unalterable opposition to greater autonomy for Ireland but because they could no longer tolerate Gladstone's political methods.

Jenkins's book will not be received as the definitive study of the Liberal crisis. There can be no such study. It is not a question of who has the most authoritative command of the sources (in this respect, surely Jenkins is the inferior of no one). Varying interpretations derive from divergent readings of the evidence, which itself cannot be conclusive in relation to the critical issue of motivation. That being said, it will be generally acknowledged that all future students of the subject will have to reckon with Jenkins's formidable achievement.

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W. C. LUBENOW. *Parliamentary Politics and the Home Rule Crisis: The British House of Commons in 1886*. New York: Clarendon Press of Oxford University Press. 1988. Pp. vii, 389.

The dramatic Home Rule crisis of 1886 has long fascinated British historians. J. L. Hammond, in *Gladstone and the Irish Nation* (1938), saw the crisis as proof of William Gladstone's noble commit-

ment to freedom and his elevation above the pettiness of his contemporaries. A. B. Cooke and John Vincent, in *The Governing Passion* (1974), saw the crisis in terms of the complex interaction of prominent politicians subtly manipulating an open-ended parliamentary situation. Now W. C. Lubenow puts the House of Commons of 1886 under detailed statistical scrutiny. His study is an intricate analysis of division voting patterns in relation to the partisan, social, and constituency backgrounds of M.P.s.

The great strengths of such statistical analyses are often of a negative kind. Quantitative testing of different interpretations can show what was not occurring. Thus, Lubenow proves that the Home Rule crisis of 1886 was not a social conflict. Social background had little to do with policy preferences. And the crisis was not a conflict of economic interests, it was not a conflict between urban and rural communities, and it was not the product of constituency influence over M.P.s. Home Rulers and Unionists both represented a variety of types of constituents, though organization and region were factors. The Home Rule crisis did not, in itself, force Liberalism into the "Celtic fringe." This was already occurring. And the Liberal schism of 1886 was not a split between experienced moderates and radical newcomers to Westminster. Rather, Lubenow reveals a crisis rooted in the ideological nature of parties inside Parliament. Indeed, he stresses the insular character of Westminster as a close-knit community whose rituals domesticated and tamed dissident behavior.

Lubenow's analysis of division voting is both important and intriguing. He exposes consistent ideological dimensions to most Commons divisions in 1886, which were largely unaffected by the crisis over Home Rule. The Conservative and Irish Nationalist parties were very cohesive, the Liberal party far less so. Most dissent within Liberal ranks assumed the form of radical rebellions following the major voting dimension of the party. The Liberal schism over Home Rule was a centrist revolt violating the normal dimensions of Liberal voting. In fact, the cleavage of opinion over Home Rule was dissimilar to Liberal differences on all other issues, including alternative aspects of Irish policy, except, to a limited extent, voting on disestablishment for Wales and Scotland. Thus, the Liberal Unionists emerge as a single-issue faction with more in common with Gladstonian Liberals than with their new Conservative allies. This very brief summary gives some indication of the significance of Lubenow's findings. Any historian approaching this period will have to take account of this study.

But Lubenow's work will not close discussion on 1886. The question of the meaning of such find-

ings will continue to exercise historians. One can demur at some of Lubenow's own interpretations. To dismiss the notion of a party purge in 1886 because Gladstone was too imprecise and guarded a politician is to confuse forthrightness with simplicity. Is the discovery of consistent ideological voting as antithetical to "high political" concerns at the level of policy making as Lubenow suggests? The question remains why Gladstone, at the exact moment he chose, introduced the specific Home Rule that he did. But these, and other questions one might have about the precise meaning of Lubenow's analysis, should not detract from the great service he has performed. By mapping out the ideological terrain, Lubenow has provided an invaluable guide for those searching for political purpose in the parliamentary labyrinth of 1886.

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JUDITH MOORE. *A Zeal for Responsibility: The Struggle for Professional Nursing in Victorian England, 1868–1883*. Athens: University of Georgia Press. 1988. Pp. xvii, 214. \$23.00.

During the past few years, feminists have discovered that nursing composed an important part of feminist history in the late nineteenth and the first part of the twentieth centuries. This discovery is important because feminists of the late 1960s and early 1970s tended to denigrate nursing as representative of all that was wrong with women's occupational roles in the past. Judith Moore is one of the new generation of feminists who argue that nursing was historically important in plowing new ground for women, and she emphasizes that this was not an easy task.

She examines in some detail the conflict between nurses and lay administrators at King's College Hospital in 1874, between nurses and physicians at Guy's Hospital in 1880, and between physicians and administrators on one side and nurses on the other at King's College Hospital again in 1883. In essence, these struggles represent to her symbolic battles of the efforts of the male-dominated medical profession and the emerging one of hospital administration to keep women (nurses) in place and under their control, even if this threatened patient welfare. She indicates how the medical journals such as the *Lancet* and the *British Medical Journal* gave decidedly one-sided and nonobjective accounts of the basic issues and how sometimes even *The Times* itself failed to report the basic issues. Physicians, in their own minds, believed the problem with nurses was that they were women and did not understand either their place or the workings of the real

world. In analyzing these struggles, Moore finds that the physicians were attempting to use the nurses to elevate their own status and power, which was considerably inferior to those of other professions of the time. The nurses, in their own ladylike way, gave as good as they got, and, although they ultimately did not emerge victorious, they also did not quite lose and came back to fight again. The one criticism I have of the book is that Moore appears unaware of the weapons used in the later phases of the struggle, which tended to avoid direct confrontation but turn on game playing such as the "doctor-nurse game." Analyses of these struggles have been much more a part of American, rather than British, writing on nursing history, and she seems unaware of them. Her case studies emphasize, however, that one of the frontiers of the battle for greater equality for women was in nursing. Her data are heavily based on manuscript sources, including hospital records and the personal letters and records of St. John's House (many of the nurses belonged to the Anglican sisters of St. John's) and of individuals involved in the controversy. Using these she can correct many of the biases in the printed media of the time.

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JAMES A. SECORD. *Controversy in Victorian Geology: The Cambrian-Silurian Dispute*. Princeton: Princeton University Press. 1986. Pp. xvii, 363. \$49.50.

Controversy, in science as in other scholarly domains, ought to be a sign of intellectual vigor, movement, and general good health. This was certainly true of Victorian geology, although, as James A. Secord also demonstrates, its practitioners were often at pains to disguise their science in more placid garb.

This study is the first, and will doubtless remain the definitive, book-length analysis of one of the nineteenth century's most intense geological wars. It was waged over where the boundary should be drawn between the stratigraphic layers of the Cambrian and Silurian systems—the oldest strata then known to contain fossils (and even today the earliest known strata to contain abundant and widespread organic remains). In retrospect we might expect such a dispute to have been significant because of its intimate connection with the question of the origin of life, or at least of life's first appearance in the geological record. But pre-Darwinian geology was not a temporal, historical science focused on origins and causes as much as it was a descriptive exercise, like the rest of

natural history, devoted to naming and sorting. Secord shows, accordingly, that among the complex motives that fueled the dispute for more than sixty years the question of the origin of life rarely surfaced. More important were issues of stratigraphic classification and nomenclature, of geological methodology and ethos, of scientific priority and property, of professionalization and patronage. It is thus a rich story, with many lessons in the ways of science, and Secord tells it well.

Although the source of the controversy was the extensive but problematic sequence of paleozoic strata in Wales, the locales of the debate were the Geological Society of London, the annual meetings of the British Association, the geological journals (and, when they failed, the popular media), and the personal correspondence and diaries of the disputants. Secord draws especially heavily on the last, as he must, for the controversy eventually became too acrimonious for public display in the judgment of those who controlled the channels of scientific communication. The future of geology, as a science free from scriptural domination and as a profession worthy of government support, seemed to demand the minimization of obvious disagreements among the leaders of the discipline.

And the principals in the dispute were indeed among the elite of the era: Roderick Murchison, "king of Siluria," outgoing gentleman amateur, later chief of Britain's Geological Survey, and symbol of the hegemonies of metropolitan geology and the paleontological approach to mapping, versus the Reverend Adam Sedgwick, Woodwardian Professor of Geology at Cambridge for no less than fifty-five years and the beleaguered advocate of the Cambrian system and an older approach emphasizing lithology and structural geology. The episode opened with their amicable, collaborative mapping tour of Wales in 1834, reached a peak (and a rupture) in the mid-1850s with what Secord calls "the Battle of May Hill" (chapter 8), and was resolved after their deaths when the Cambrian and Silurian systems were found to be divided not by a single unconformity but by another entire system of strata.

Secord's treatment of the geology itself, supported as it is by numerous charts, maps, sections, and fossil illustrations, is detailed, careful, and at times tedious for all but the dedicated stratigrapher. Historians will appreciate, however, that the geology is finely balanced by analyses of the social and epistemological dimensions of the debate. Especially fascinating are the discussions of how scientific property (in this case, portions of the geologic column) came to be defined through maps and nomenclature; how geological systems or eras like Cambria and Siluria became "potent symbols of distinctive values in science" (p. 241);

how geological myth making was carried out and patronage deployed by both sides in the dispute; how geological truth was certified increasingly by the young professionals of the Geological Survey; and how controversy, though regarded as pathological to the image of science by Victorian geologists, was in fact the catalyst of both new discoveries and eventual consensus. Future scholars of scientific controversy may well regard this study as an exemplar for the genre.

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WILLIAM P. KENNEDY. *Industrial Structure, Capital Markets, and the Origins of British Economic Decline*. New York: Cambridge University Press. 1987. Pp. xii, 230. \$49.50.

William P. Kennedy addresses the relationship between the relatively small share of new "strategic" industries and Britain's late nineteenth-century growth performance. This is an important issue, but this book unfortunately retards understanding. A considerable literature is reviewed, but a set of calculations that purport to illustrate that a more modern economic structure would have raised British output in 1913 between 26.8 percent (p. 65) and 271.5 percent (p. 72) represents the original contribution. These calculations are without foundation. They rest either on a peculiar implicit assumption or, more likely, on a programming error. This book from a major publisher with full scholarly trimmings will mislead the reader as Kennedy has been misled.

It is important to realize that Kennedy has not reached his conclusion by analyzing productivity differences among sectors or between countries or by explicitly modeling capital formation and skill acquisition. Rather he believes he has calculated a new level of output that is implied by a different sectoral structure of the economy. This is false.

A moment's reflection will indicate that there are unlimited combinations of sectoral growth rates that can result in new shares. At one extreme all sectors could decline rapidly with old sectors declining more rapidly than the new. At the other, all sectors could grow very rapidly, but the new sectors could grow faster. Any aggregate growth rate is consistent with changed shares.

Kennedy's estimates are thoroughly described in his text (pp. 16–21) and appendixes. First he inflates the 1913 output of each sector in proportion to the counterfactual increase in shares and then calculates sectoral growth rates. These growth rates are then multiplied by sectoral shares and summed to provide an estimate of aggregate

growth and then aggregate output. Because this output exceeds actual output, he concludes that the initial sectoral increases were insufficient to generate the new structure and so inflates each final output by the calculated percentage increase in aggregate output and recalculates. "Recalculations are repeated until . . . repeated iterations converge to a limiting value" (p. 20).

This procedure does not work. Kennedy's method of moving to and from growth rates both obfuscates and adds imprecision because the weighted sum of sector growth rates is an approximation rather than an exact measure of aggregate growth. The second round of Kennedy's procedure is to inflate both "old" and "new" by 3.75 percent. "Total" will increase by 3.75 percent, and the process will never converge.

Where, then, do Kennedy's numbers come from? Careful examination of the computer algorithm (pp. 174–76) reveals that the "rest of the economy" whose share is .4811 percent is treated differently from sectors whose shares are changed in the counterfactual calculation. Instead of its actual output being inflated, its growth rate is calculated as the sum of the growth rate times the share of the other sectors. While I assume the intent was to have the "rest of the economy" grow at the same rate as total output, this calculation makes this large sector grow at 52.89 percent of the average growth of the other sectors. This allows the calculation to converge but seems without intellectual justification.

There is somewhat more in the book. Sectoral performance is reviewed, and the characteristics of the British financial system are discussed. The work must, however, stand or fall on its central calculations. It falls.

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BARRY SUPPLE. *The History of the British Coal Industry. Volume 4, 1913–1946: The Political Economy of Decline*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xxv, 733. \$98.00.

Although deploying 696 pages of weighty text, Barry Supple's tome is not a full history of the British coal industry in the crucial years spanning the two world wars. He admits that there is relatively little detailed coverage of technological innovation, work processes, industrial relations, or the sociology of mining communities; rather, he concentrates on the field of "political economy," the extent to which the state of the industry became a national political issue as the century went on. Supple accepts that this perspective is oversimplified and very much his own. Yet the

series in which the book appears was sponsored by the state-owned National Coal Board (now British Coal), and the author clearly relies heavily on the work of his two research assistants.

It must, however, be said that none of these points weakens the contribution made by the volume. There are already many specialized works on all aspects of mining history in Britain; it has proved a fruitful seam for scholars and ideologues alike. Supple offers more than a synthesis. His interpretation gives what could be an unmanageable and massive book a distinct and crisp shape and one that, as he says, links coal with the larger story of Britain's modern industrial, economic, and political history. Also there can be no doubt that the author's ideas were given free rein; he holds no brief for the behavior of government, employer, or union during those troubled years.

We are presented with a lucid portrait of a staple industry under pressure, no longer able to maintain its leading position within the pre-1914 world, when there were over a thousand mines and over a million miners in the coal-rich island. Foreign competition and alternative sources of energy created a chronic problem of overcapacity; the response revealed nearly fatal cracks in the structure of the industry. The coal-owners—of a vast number of small and medium-sized firms—eschewed reform in favor of reaction, refusing rationalization and seeking merely to cut wages. The miners, too, were forced to compromise only after the bitter and disastrous strikes of 1921 and 1926 and the mass unemployment and migration from the coal fields that followed. The (usually Conservative) governments applied pressure for compulsory reorganization until the exigencies of war forced control on them. All in all, there was too much of a determination to confront common challenges.

There are many useful insights to be gained from Supple's efforts. He admits the regional differences that countervail the general portrait of decline: Yorkshire and Nottinghamshire had a future in mining; many other fields did not. The brief treatment of the miners' fascinating social history concludes that their villages were "neither hungry nor healthy." The miners' response to both world wars gives the lie to the sentimental notion that all Britons then pulled together, uncomplainingly.

Yet the chief value of the book lies in its broader sweep. It explains much about Britain's situation in the first three quarters of the twentieth century: its perception of its decline as a world power, the rise of union influence, of conflictual industrial relations, and of state intervention, eventually to the point of nationalization. It would all seem depressingly in the mainstream of British history

were it not for the prospect that the Thatcher years might just reverse all of these trends. And, as an application of her clear if narrow ideology, might the rump of the coal industry be about to return to private hands? Supple's admirable volume, potent a study of decline as it may be, is at least not literally the last word on the history of British coal.

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HUGH B. PEEBLES. *Warshipbuilding on the Clyde: Naval Orders and the Prosperity of the Clyde Shipbuilding Industry, 1889-1939*. Edinburgh: John Donald. 1987. Pp. v, 205. \$45.00.

In barely one hundred thirty pages of text, Hugh B. Peebles has traced the history of warship construction in the west of Scotland from its origins in the middle of the nineteenth century to the outbreak of the Second World War. Throughout most of the period, the Royal Naval dockyards were the principal suppliers of British warships; private yards were employed only when orders exceeded the capacity of the dockyards. As this situation tended to occur in brief but well-spaced bursts when changes in naval technology or worsening international relations alarmed an otherwise penny-pinching Parliament, tendering for warship orders carried significant risks. Although many private yards initially hoped to use naval construction to help them through lean periods when merchant ship orders were scarce, they soon found that working for the navy, which often required large capital investments, in fact limited their options.

Warship building on the Clyde, the most important shipbuilding river in the world, was confined to a few firms, the most important of which were the Fairfield Shipbuilding and Engineering Company, the Clydebank yard of John Brown and Company, and the Dalmuir yard of William Beardmore and Company. By the early twentieth century, all of these yards—in common with such major competitors as Vickers, Cammell Laird, and Armstrong-Whitworth—were linked to vertically integrated armaments concerns that produced armor plate and artillery as well as ships. Their merchant work was largely restricted to liners, which demanded the same high quality of workmanship and extensive facilities as warships. Because they had become so highly specialized, these yards were especially vulnerable in downturns because they could not easily turn to speculative construction as smaller yards could.

Peebles has based his work on a detailed exam-

ination of the surviving records of the major Clyde concerns. Despite the (relative) breadth implied by the title, the book is really a financial history of the warship builders that traces the interplay of their private and naval contracts. As such, it is a most valuable contribution to the business history of shipbuilding, but it is unlikely to appeal to the general historian. Peebles says very little about government policy toward private builders and virtually nothing about changes in the technology of either warship construction or ships themselves. Although enthusiasts will be attracted by the twenty-eight-page appendix giving details of all Admiralty orders executed on the Clyde between 1859 and 1939, the overall impression of the study is curiously disembodied.

It is obvious from Peebles's tables and notes that he has collected a tremendous amount of useful information on both naval and merchant shipbuilding. It is to be hoped that he will use this data in a second book to illuminate a larger share of the history of this very important industry.

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PATRICIA HOLLIS. *Ladies Elect: Women in English Local Government, 1865–1914*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xx, 533. \$74.00.

Studies of women's struggle to obtain and to exercise formal political power have focused on the national franchise and Parliamentary politics. Relatively few people are aware that many English women "had the vote" and entered the political arena in local government long before their national enfranchisement in 1918. Although women who were active in Victorian local government are relatively familiar figures to students of such topics as the Poor Law and education, the absorption of those scholars with their topics, rather than with women, has supported the tendency to see the female local politician as an isolated, idiosyncratic player on a larger male stage. Patricia Hollis's imposing and absorbing book redresses the balance by making women, rather than political issues, the focus of this study of local politics.

Although married women were excluded from the local franchise, unmarried female ratepayers held the vote with the same property qualifications as men. In Victorian England they made up between 17 percent and 25 percent of the local electorate. Those female voters were not just rich Tories or high-minded Liberals but women of all kinds and classes, from wealthy unmarried women to widows struggling to bring up young families and working women with small shops, laundries,

and other businesses. Although a large proportion of the women elected to local office were prosperous and middle class, others were of modest means and espoused working-class causes.

Although there was a close connection between philanthropy and local government, these women did not seek simply "to do good." They wanted to try to solve the practical social problems of their day, not at the level of high principle or theory but in the back-street, the schoolroom, and the workhouse. Nor were their motives purely altruistic; many of them were impelled by the drive to exercise their right "to have a say," to participate in public affairs, and to be full citizens. Although Hollis takes a case-study approach to her subject, detailing the lives and work of dozens of women representing a great variety of constituencies throughout England, she uses these portraits to develop and illustrate the major themes of women and public life in the nineteenth and early twentieth centuries. She argues, persuasively, that "philanthropic and suffrage women . . . converged in late Victorian England on local government" (p. 461), and Hollis sees local government as a place of power in its own right as well as a stepping stone to national politics. There are subtle and well-balanced examinations of the relationship between men and women in the pragmatic world of local politics, of the affiliations between feminism, philanthropy, and socialism at the local level, and of the impact of local government on different groups of women through its roles as a source of political power, a provider of services, and an employer.

The ladies-elect struggled constantly in both the margins and the mainstream of politics and social change. Many devoted their lives, and considerable energies, to local politics, and there was no real shortage of women candidates. The problems lay in women being nominated by local parties for winnable seats, rather than certain failures, and in their reelection. Women in nineteenth-century local government faced the issues that continue to confront women in public life: how to reconcile the support of "women's issues" with an involvement in traditional "male" concerns and how to use their strengths as women to enhance their power in the predominantly male public realm. Although they were subject to the sexism of many of their male colleagues and constituents and often relegated to certain failure or "powerless" positions, these women did not always play a submissive role. As Linda Gordon has recently shown (for a very different place and circumstance), women have not been the helpless victims of "the system" but have manipulated it in subtle though often powerful ways to enhance their authority while retaining their own style and serv-

ing their own interests (Gordon, "Family Violence, Feminism, and Social Control," *Feminist Studies* 12 (Fall 1986): 452-78).

Hollis has drawn on a wide range of sources and provides an excellent bibliography to guide others into this topic. The book is written with grace, good humor, wit, and style and provides a model for work in the field. Far from being limited by the parochialism that might seem implicit in its title, the book is a fascinating and wide-ranging study that should be read by all serious students of late nineteenth- and early twentieth-century social, political, and women's history.

JANET ROEBUCK
University of New Mexico

BILLIE MELMAN. *Women and the Popular Imagination in the Twenties: Flappers and Nymphs*. New York: St. Martin's. 1988. Pp. x, 196. \$25.00.

This modest book should not be overlooked by any scholar interested in popular culture or in modern history. In a succinct but thoughtful account, Billie Melman traces the rise and decline of the flapper, "a figure characterised as sexless but libidinous; infantile but precocious; self-sufficient but demographically, economically and socially superfluous; an emblem of modern times yet, at the same time, an incarnation of the eternal Eve" (p. 1). Drawing her evidence from a wide variety of sources from the years 1918-28, Melman shows that the flapper was the subject of numerous debates in Parliament, in the mass-circulation newspapers, among the intelligentsia, in popular novels, and in pulp magazines. Women have, of course, been complex symbols of an age before, but Melman cogently demonstrates the ways in which the flapper could uniquely evoke freedom for women, sexual fear for men, and political instability for the press. Indeed, the most egregious commentators connected the androgynous flapper with the disasters of World War I and the Great Depression.

Melman is at her best in exploring the richly evocative world of the popular novel. The best-selling "sex novel" of the 1920s featured a contemporary heroine, gave a more or less realistic presentation of a woman's sexual experience, and openly discussed feminine sexuality. The exotic settings, slangy style, and confident heroines attracted a largely female readership. Most critics found the frank sexuality in these best sellers to be a sign of society's postwar degeneration, but a closer look reveals that their daring subject matter was cast in a familiar romantic mold.

The most enduring sexual image of the 1920s is from the cinema: Rudolph Valentino in *The Sheik*

(1921). The film, based on a novel published in 1919, and its male star were part of a decade-long enthusiasm for passionate Arab lovers and aristocratic women. Melman concludes that the desert novel is an example of semipornographic fiction that catered to an almost exclusively female audience; as such it, along with the films and records, "represent[s] one of the earliest examples of mass, commercialised erotic literature" (p. 104). More controversially, she argues that this fiction is evidence that pornography is not an exclusively male domain.

A third section discusses the realistic fiction written for pulp magazines aimed at young working-class women. Although a good many generalizations have been made about the rise of mass-circulation magazines, few critics have read the fiction, editorials, and letters to the editors with as much care as Melman has. Her shrewd analysis of "factory lass" stories shows how this fiction sustained traditional working-class values of community, honesty, and hard work. But here again, Melman concludes, the fiction was as much a created fantasy as were the best-selling novels of high life and remote lands.

Although the author is aware of the dangers of inflating her own subject, she does not always place it within a larger context. For example, our understanding of the public debate about the "sex novels" would have been enriched by a comparison with a similar debate in the 1890s. More could also be said about the class bias of the flapper symbol and how this changed over the decade. But these criticisms only ask for further information from a richly intelligent and provocative book. It is cultural history at its best.

MARTHA VICINUS
*University of Michigan,
Ann Arbor*

BRIAN HARRISON. *Prudent Revolutionaries: Portraits of British Feminists between the Wars*. New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 362. \$64.00.

Studying the "antis" for his pathbreaking analysis of the British antisuffrage movement, *Separate Spheres* (1987), Brian Harrison garnered much of the material for the present volume. A splendid index and magnificent photographs enhance Harrison's massive archival research and the interviews he conducted with the descendants, friends, and colleagues of the women who have become familiar names to students of British women's history. Reading this book, we empathize with his enjoyment of discussing the organization women Pippa and Ray Strachey and Eva Hubback, the

Pethick-Lawrences and their model marriage, the ideas of Theresa Billington Greig, the international diplomacy of Margery Corbett Ashby whom Harrison interviewed when she was in her nineties, the Labour M.P.'s Margaret Bondfield, Susan Lawrence, and Ellen Wilkinson, and the distinguished parliamentarian Eleanor Rathbone.

In all, Harrison portrays fourteen women and two men. He specifically omits, for reasons that are not completely clear, a handful of other individuals, for example, Mary Stocks and Helena Swanwick and the members of the well-known Six Point group, whose "feminist" contribution would have merited inclusion.

In a book of some three hundred pages, however, one can only do so much. Harrison decided to concentrate on the relationship between feminism and Parliament (p. 3). He argues that mandatory party allegiance often forced women M.P.'s to vote against their feminist instincts. He begins by setting out the contrast between the constitutionalist Millicent Fawcett and the militant Emmeline Pankhurst. These two women, personifying prudent realism and impatient idealism in Harrison's view, set the stage for the ensuing feminist divide during the first half of the twentieth century. Clearly he favors prudent realism (Rathbone) over impatient idealism (the Six Points group). He develops his theme by describing the successes and failures of his subjects in their quest for parliamentary seats and the founding of organizations to promote improvements in women's lives. Thus, Rathbone, following Fawcett as the leader of the constitutionalists, spoke up in the House as early as 1933 against the danger of fascism and became the most persuasive parliamentarian. Pitting her ideas against conventional liberal feminism, which sought equality of opportunity in education and the professions, she achieved in a brilliant diplomatic campaign the broadening of a "new" feminism to encompass rights of women as mothers through family allowances. This stamps her as one of the "revolutionaries" of Harrison's title.

Although the word "feminist" is part of the subtitle of his book, Harrison does not specifically define what he means by feminism, but he is aware of the ambiguity of the term in the interwar years (pp. 311–13). Like the men of Virginia Woolf's circle, he dislikes her major philosophical exposition of this subject in *Three Guineas*, calling it "bitter and narrow" (p. 97). In passing, however, he gives us a clue to the problem of feminist development when he cites Rathbone's encouragement of Townswomen's Guilds toward *visible* productive work (p. 196). Implicitly Rathbone understood that the real revolution of feminism would be infinitely gradual and invisible.

In Harrison's view these feminists between the wars were considerably successful in achieving a new society—transforming not merely a political system but details of domestic life (p. 322). To some extent this is so, but Harrison's dislike of the militants and the "not so prudent" leads him to underestimate their function and the significance of their contribution to this achievement.

SUSAN GROAG BELL
Stanford University

MARK SHIPWAY. *Anti-Parliamentary Communism: The Movement for Workers' Councils in Britain, 1917–45*. New York: St. Martin's. 1988. Pp. xiv, 239. \$49.95.

This book is the first detailed, well-researched account of the workers' council, or "council" communist, movement in Britain, mainly on the Clyde-side, from 1917 to 1945. It is a history not of British syndicalism but of two antiparliamentary groups—the Workers' Socialist Federation, or Dreadnought group, launched by Sylvia Pankhurst, and the Glasgow Anarchist Group. The Glasgow organization underwent a name change in 1920, becoming the Glasgow Communist Group to show sympathy with the Russian revolution. A year later it became the Anti-Parliamentary Communist Federation (APCF) to distinguish itself from the Moscow-backed Communist party of Great Britain. Guy Aldred was the heart and soul of this group, editing its newspaper, the *Spur*, that propagated an anarchist version of antiparliamentarism.

Mark Shipway traces the sectarian squabbles and divisions among these and related groups. A major split occurred in 1933–34 when Aldred broke away from the APCF to form the United Socialist Movement, and a second schism, headed by Frank Leech sparked by disillusion with the conduct of the Spanish Civil War, led to the formation of the Glasgow Anarchist Communist Federation, which became part of the Glasgow Group of the Anarchist Federation of Great Britain during World War II.

The APCF presented a genuine alternative to capitalism—a utopia without states, classes, money, and wages. That utopia was to be achieved by agitating class-conscious workers to form workers' councils and somehow overthrow capitalism. Antiparliamentarians called their alternative vision socialism or communism, disdaining the policies of the British Labour party, which they considered a capitalist organization, and Russian-style communism, which, earlier than most, they perceived as state capitalism.

One of the book's defects is the decision of the author or editors not to provide more biographi-

cal background on the personalities involved, especially Aldred, whose role is so important to the story. Shipway does refer the reader to John T. Caldwell's article in *Essays in Scottish Labour History* (1978), edited by Ian MacDougall. There one learns that Aldred developed his antiparliamentary convictions firsthand while serving as parliamentary correspondent for *Justice*, the organ of the Social Democratic Federation. By 1905 he was writing for the *Voice of Labour*, the organ of the Industrial Union of Direct Action. Three years later he became friends with Sir Walter Strickland, a self-exiled aristocrat and friend of suppressed nationalities who in 1938 left Aldred enough money to run the Strickland press for the rest of his life. To some, these debits may seem superfluous, but others may find the facts add dimension and meaning to the story.

Anyone familiar with the history of anarchism is aware of its divisions between pacifist and violent schools, between local and national organizations, and between individual agitation and the creation of alternative communes. It should come as no surprise that the movement died during the post-World War II boom amid rising standards of living, low unemployment, and wider provision of social welfare, its minuscule audience dispersed by suburban housing schemes.

Eventually antiparliamentary anarchism had a resurrection during the late 1960s and early 1970s among students, Committee for Nuclear Disarmament demonstrators, pacifists, hippies, squatters, members of punk culture, and the alternative society counterculture. Lacking a systematic ideology, united mainly by opposition to materialism and authority and hampered by excessive individualism, antiparliamentarism failed once more to attract trade unionists even in a post-Karl Marx ethos. Eschewing government, antiparliamentarians were restricted to the streets or short-lived underground papers to influence events.

In 1964 the Anarchist Federation of Britain was revived and expanded until it died in 1974 shortly before the appearance of the Wildcat group, an umbrella organization of libertarians, who helped Shipway edit his final text. As a member of the Wildcat group, Shipway's interest in his subject comes quite naturally, and he writes with unwavering commitment to socialist principle.

NORBERT C. SOLDON
West Chester University

STUART SILLARS. *Art and Survival in First World War Britain*. New York: St. Martin's. 1987. Pp. xi, 192. \$29.95.

How the experience of World War I during a single year—1916, “the emotional heartland of the war” (p. 9)—was conveyed to the British public by “popular” writers and graphic artists is the subject of this book. The major problem faced by artists was that the war was so gruesomely new in its technology and awesomely total in its destructiveness that there were not at hand adequate forms capable of rendering it realistically. The challenge, then, for artists and writers, was either to forge new traditions or to fall back on the antiquated forms derived for the most part from outworn “heroic” or chivalric matrices hopelessly anachronistic for modern war. With but few exceptions, the choice of antiquated forms proved to be the rule. During the war years literature and art in Great Britain largely functioned as an adjunct of propaganda.

This book is an examination not of what happened but of how what happened (and what did not happen) was recorded for the British public. Adding to the problem of artists, who had to convert the titanic complications of humanity at war into recognizable artifacts, both the Admiralty and War Office blocked artists' access to theaters of operation for fear that what they might learn and express would provide moral support for the enemy. So restrictive were these policies that the official Press Office was promptly dubbed the “Suppress Office” by frustrated correspondents. “Writing fellows” were objects of derision and distrust. Whenever possible, generals and admirals wanted their own staffs to prepare press materials—often with unfortunate effects. Admiralty reports of the Battle of Jutland, for instance, actually damaged public morale by understating German losses and implying that the Royal Navy had taken more bruises than it actually had.

Stuart Sillars includes chapters treating warfare on land, on sea, and in the air. Although his discussion of better-known figures such as Paul Nash and Wilfred Owen is pedestrian, he breaks new ground in demonstrating how comparatively trivial episodes could be elevated into “icons of heroism” for the purpose of encouraging support of the war effort. He makes the telling point that major crises such as war demand mythic distortion rather than historical fact. A case in point is the saga of John Cornwell, a boy-gunner mortally wounded at Jutland, who manned his gun after all his mates had been killed. He quickly came to embody, in the popular press, “the spirit of Old England,” and his story—with graphic representations—was highly publicized in order to shame idlers and shirkers. Jutland itself was less palatable and comprehensible to the public. Similarly, the Somme disasters, which resulted in no significant strategic advantages to justify the carnage, were

glossed over by emphasizing the immense bombardment that had preceded the battle, proof positive that national unity had been at long last achieved through the employment and integration of artillery paraphernalia. Further, the war in the air became a reenactment of medieval tournaments with "knights errant" fighting "duels in the sky." Even the arrival of Zeppelins over England, which brought the war home to civilians for the first time in centuries, had to be limned within the context of Lucifer's fall in John Milton's *Paradise Lost*.

After witnessing the panoramic horror of the American Civil War, Walt Whitman warned that "the real war" would not get into the books because the reality was so much greater than the artist's capacity to evoke it. How does one place the noise, the stench, the boredom of war on a canvas or on a page? Confronted by war, all art must fail, and Sillars shows the parameters of this failure in his well-researched and thought-provoking book.

CECIL D. EBY

University of Michigan,
Ann Arbor

STUART WALLACE. *War and the Image of Germany: British Academics, 1914–1918*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1988. Pp. x, 288. £20.00.

The mobilization of academics and intellectuals for national war efforts has been a notable feature of twentieth-century international conflicts, whether "hot" or "cold." International rivalries were quite capable of drawing some well-known scholars into partisanship even in the late nineteenth century, but those conflicts were also balanced by a growing dominance of international standards of research and scholarship as well as by a trend toward international scientific congresses down to World War I.

The onset of the First World War caused scholars in many combatant countries to lend their skills to the national effort or, in many cases, to become suspect and even persecuted if they did not. The overwhelming support for the German war effort by that country's professoriate has been excellently documented by Klaus Schwabe in his *Wissenschaft und Kriegsmoral* (1969), but until now there has been no attempt to write a definitive work on the similar process in Great Britain.

Stuart Wallace has set out to provide such a judicious, critical, and well-researched work, and he has largely succeeded. Noting the smaller size and perhaps greater social cohesion of Britain's "intellectual aristocracy" (as Noel Annan has described it), Wallace proceeds to argue that the

divergent reactions of British academics to the war and especially to Germany, still regarded by many of them in 1914 as the homeland of modern science and scholarly method, changed this cozy world forever.

By "academics" Wallace means in general the roughly two thousand teachers at universities and polytechnics (almost half of them still at Oxford and Cambridge) and of that number chiefly those in the humanities, especially history and philosophy. The book is a valuable collection of statements and arguments developed by many dozens of these, although not all receive equal weight. Three—Gilbert Murray, G. L. Dickinson, and Bertrand Russell—even receive separate chapters. The author clearly sympathizes with the plight of those academics whose careers were damaged by their questioning of wartime policies, but he does not attack those who chose to follow a more clearly "patriotic" line. The central theme is that of the intellectual conflict felt by academics as they tried to find where their duty and their conscience lay.

I was struck by some weaknesses as perhaps other readers will be. The book is not very clearly structured, and chapters (as well as the book) end without summary conclusions. The various academics are presented in a mosaic style, and the background structure of British academic life is left largely untouched. Individuals and their opinions, discipline groups, and controversial topics form the core of the various chapters. On the other hand, a useful bibliography and six appendixes add to the usefulness of this volume as a reference work. Only in a two-page postscript does the author inform us that a pervasive sense of conservatism, not outright persecution (as in the Russell case) or even fear of losing academic jobs, produced the reactions of the majority of British academics toward Germany during World War I.

Thus, this work does not equate with Schwabe's definitive study of the German side, and it remains restricted largely (though not exclusively) to views of Germany. But it is a welcome and well-researched guide to the subject that should remain very useful to students of the relationship between intellect and power in this century.

CHARLES E. MCCLELLAND
University of New Mexico

BRIAN HOLDEN REID. *J. F. C. Fuller: Military Thinker*. New York: St. Martin's. 1987. Pp. xiii, 283. \$32.50.

Brian Holden Reid has built on the recent biography of General J. F. C. "Boney" Fuller by Major-General A. J. Trythall and has leaped right into an intellectual analysis of Fuller and the provocative English pundit's writings. Reid shows us that

Fuller was not just a military writer, as many may have believed, but also a Darwinist straight out of his nineteenth-century education, who had a belief in the occult and in psychology and who ultimately dabbled in fascism, all for the purposes of making it plain that, if peace was a complex matter, so naturally was war.

Reid bases his work on a large collection of papers that have become available only within the last twenty years in the Liddell Hart Collection at King's College, London, in the Public Record Office, and elsewhere, and he uses a wide variety of printed sources, in many cases book reviews and memoirs. He has been rather lucky in his ability to trace the intellectual background of a military man in that Fuller was close friends with B. H. Liddell Hart but often separated from him so that there is a wealth of correspondence between these two intellectuals who had an immense influence in their own lifetimes. What is also fortunate is that Fuller was such a character and irritated people so much that he was just the sort that everyone would comment on if they wrote much at all, even if they read practically nothing. Tim Travers in his recent work *The Killing Grounds* (1987) has shown how the army in the First World War removed people like Fuller. Fuller was saved from this because he had not yet become that difficult or that senior before the war ended.

Reid takes the reader through the periods of Fuller's intellectual development and shows how his writing reflects at first the environment in which he worked but then increasingly the broader world that he saw and the clash of armies that he again anticipated. For each period after the end of the First World War and for Fuller's notable Plan 1919 for a major tank breakthrough, which might or might not have worked as the general was no technician, Reid examines one or more of Fuller's many books. There is a final chapter that covers the period from the beginning of the Second World War to Fuller's death in 1966.

This is a book for both intellectual and military historians. It is thoughtful, explanatory, and at times arrogant.

ROBIN HIGHAM
Kansas State University

HAROLD R. WINTON. *To Change an Army: General Sir John Burnett-Stuart and British Armored Doctrine, 1927-1938*. Foreword by PETER PARET. (Modern War Studies.) Lawrence: University Press of Kansas. 1988. Pp. xviii, 284. \$29.95.

Harold R. Winton has written an exceptionally well-balanced book about the problem of modern-

ization in the British army in the 1920s and 1930s. Although the focus is often on General Sir John Burnett-Stuart, in fact the book is really concerned with the perennial question of why the British army was slow to mechanize in the interwar period. Winton commences by pointing out that there have been three explanations for British problems in mechanization: first, that the army was wedded to old concepts, organizations, and ideas and was unwilling to make dramatic changes; second, that the army never had the money or the popular support for changes in doctrine and organization; and third, that the lack of a clear Continental commitment, plus the need to garrison the empire, retarded concepts of armored warfare.

Before coming to his own conclusions in that debate, Winton argues that the problem is complex and that the army was actually split into a spectrum of attitudes toward mechanization ranging from those of the revolutionaries, reformers, and progressives to those held by conservatives, reactionaries, and the indifferent. Apparently that spectrum of response remained throughout the interwar period, though one wonders whether the same range of attitudes would have been true of other armies in the same period or, indeed, whether it was typical of the British army in the twentieth century. In his conclusion, Winton comes to a rather generalized evaluation of why British mechanization was slow, particularly after 1929, but he stresses the problem of the imperial defense mission in sidetracking the speed of mechanization. Other explanations Winton gives for the relative lack of mechanization are military conservatism, organizational inertia and rivalries, technological uncertainty, conflicting personalities, political indifference, some social hostility, and chance. In other words, Winton lumps together the elements of the three original explanations, highlighting the imperial commitment and including the element of chance. Thus, there are no simple answers, which is what one might expect given the wide-ranging ramifications of what mechanization implied in political, social, economic, and intellectual terms.

Nevertheless, within Winton's explanations, there emerges a strong theme that is perhaps undervalued, namely, the British difficulty in developing doctrine. Just as the prime weakness of the British army before and during World War I was the reluctance or inability of the high command to develop doctrine, so the same kind of confusion over mechanization and tank theory plagued the army throughout the interwar period. The main battle lines drawn were between, on the one hand, the concept of an armored formation composed mainly of tanks and capable of pene-

trating deep into enemy lines in a Continental war and, on the other hand, the concept of a motorized, or perhaps lightly armored, cavalry division capable of performing reconnaissance and screening roles for motorized infantry but not designed specifically for a European war. On one level, the debate was an argument for the value of those forces on the Continent, as against their use in imperial policing; but, on another level, it was really an argument over the internal logic of the tank, as opposed to mechanization in general. Just as the discussion over the machine gun before and during World War I really focused on the question of whether the gun was significant enough to be an independent weapon, so the role of the tank was really at the bottom of the mechanization debate. Had the tank been recognized as significant enough to be given a fully independent role, then mechanization would have conformed to the tank and not the other way around. But that basic debate was not resolved, and, without a coherent doctrine, it was difficult to produce a convincing case for mechanization. Winton summarizes the result: "Britain went to war in 1939 with neither an effective armored division nor a coherent doctrine of armored warfare" (p. 209).

One cannot help concluding, while taking into account the other serious obstacles to mechanization that Winton enumerates, that the original thesis of military conservatism propounded by J. F. C. Fuller and B. H. Liddell Hart has some merit. That is particularly the case when linked with the army's preoccupation with imperial defense. The career of Burnett-Stuart reflects that situation. As a convert to mechanization, he had frequently to fight against conservative officers such as Chief of the Imperial General Staff Montgomery-Massingberd. Yet Burnett-Stuart himself was much concerned with imperial defense and thus eventually came to different conclusions from the advocates of the tank, even while strongly supporting the concept of armored units (p. 237).

Winton has produced an excellent book that places the question of mechanization within the whole complex interaction of army, technology, personalities, economics, society, and politics and shows how difficult it was for intelligent people such as Burnett-Stuart to make choices.

TIM TRAVERS
University of Calgary

F. H. HINSLEY *et al.* *British Intelligence in the Second World War: Its Influence on Strategy and Operations.* Volume 3, part 2. New York: Cambridge University Press. 1988. Pp. xvi, 1038.

In this final volume of a study of British intelligence, strategy, and operations during World War II, F. H. Hinsley and his associates deal with the last year of the war in Europe. Among the subjects covered in the 746-page narrative are a fulsome description of Overlord and the subsequent campaigns in the West, the Soviet offensives and those of the Allies in Italy, the German withdrawal from the Balkans, the final stages of the sea and air wars, and German weapon developments. Also included are thirty appendixes, which clarify, among other topics, Poland's and France's contributions to figuring out Germany's Enigma machines, which were much greater than previously thought, and Britain's lack of foreknowledge of the plot against Hitler. The volume concludes with an outstanding, if scaled-down, bibliography, a thorough index, and a helpful list for researchers of series prefixes and delivery group designators used for signals to overseas commands. As in the preceding volumes, the authors make use not only of high-level Ultra intercepts but also of tactical intelligence, photo reconnaissance, and decoded Japanese dispatches to support their contentions.

The book's overall theme is that the amount of intelligence available to the Western allies by the last year of the war reached truly monumental proportions, but even at this point the extent to which intelligence disclosed key information and could be used in operations varied a great deal. It did not, for instance, lead most evaluators to deduce that Germany's Ardennes operation was in the offing. Nor did it give advance notice of Russia's offensive against Romania in August 1944 or of several other Soviet campaigns in 1945. It did, however, give a relatively accurate picture of the location and quality of enemy forces and equipment in the West and in Italy, and Enigma decrypts followed in detail the Wehrmacht's withdrawal from southern Greece and the Aegean Islands. In addition, the book provides an excellent synthesis of Allied operations late in the war as well as the intelligence involved, and it also corrects some errors contained in earlier published works, including several official histories. Among other revelations the authors assert that the Allies had considerable knowledge of the many new weapons the Germans were bringing on line during 1944-45; that, although there were some Ultra references to Nazi concentration camps, there were none to "extermination" camps (p. 736); and, continuing a theme brought out in volume 2, that, despite reservations, British authorities as early as the summer of 1943 "concluded that while the Germans were researching on atomic energy, their primary objective was not the production of weapons but the development

of power, and that the danger that they would acquire an atomic weapon before they were defeated could be discounted" (p. 584).

Criticisms of this magnificent study are few. The statement, repeated in earlier volumes, that the series does not attempt to cover the war in Asia and the Pacific because of the overriding interest of the United States in those theaters wears thin in view of the impact these areas had on Britain's strategic plans and actions. Although the maps are plentiful and well done, many of the place names mentioned in the text are not on the maps. And, although some sections, such as the one dealing with the Ardennes battle, are well written, others, such as those on the campaign to the Rhine and the final months in Italy, are wooden and repetitious in style. Nevertheless, taken as a whole, this volume and its predecessors, along with previously published works on intelligence in the field and forthcoming books on security and counterintelligence and strategic deception, should give us an excellent picture of Britain's central role in this elusive but incredibly important aspect of World War II. The work of Hinsley and his colleagues over a period of more than ten years represents a remarkable achievement.

ALAN F. WILT
Iowa State University

MARION BERGHahn. *Continental Britons: German-Jewish Refugees from Nazi Germany*. Paperback edition. New York: Berg; distributed by St. Martin's, New York. 1988. Pp. ix, 294. \$14.95.

Between 1933 and 1939, Great Britain offered refuge to about eighty thousand Jews fleeing Nazism in Germany, Austria, and Czechoslovakia; fifty thousand of them remained in Britain after the war. In this unrevised edition of a study first published in England in 1984, Marion Berghahn explores their emigration, settlement, and subsequent acculturation and integration, drawing primarily on a series of 180 informal interviews that she conducted with refugees and their British-born children. This is a useful account—part narrative, part oral history—of a migratory wave hitherto ignored by historians of British immigration. It is particularly successful in describing the long-term aftereffects of the refugees' involuntary uprooting from a society of which they thought they were an integral part. Many of Berghahn's respondents, for example, remain haunted by obsessive memories of degradation and humiliation half a century after their forced exodus.

Berghahn's focus, however, is broader than the above capsule description might suggest. She presents her account of refugee adjustment in the

context of historical and sociological questions about the persistence of ethnicity in modern societies. Her first chapter is a well-argued critique of the historiography of Jewish assimilation in Germany in which she attacks the view that once German Jews abandoned traditional religious practices they were on an irreversible path toward total assimilation. In doing so, she provides a helpful corrective to the once-regnant belief that German Jews, with few exceptions, exchanged their "Jewishness" for "Germanness," becoming more or less indistinguishable from their neighbors. Rather, she argues, what occurred was a redefinition of Jewish identity, a transformation of what being Jewish meant rather than a disintegration of prior loyalties, sentiments, and perceptions. None of this, however, is as novel as the author believes. Shulamit Volkov and Henry Wassermann, with whose work on this subject Berghahn appears to be unfamiliar, have published a number of articles in the past decade arguing exactly this point.

Berghahn concludes her chapter on Jewish assimilation in Germany with a plea for more subtlety in understanding ethnicity in the modern period, arguing that mental structures rather than cultural habits and practices primarily characterize ethnic differentiation in plural societies. Yet in the chapters that follow the author only partially heeds her own call for this kind of approach. She shows that both the refugees and their children, despite their outward success in adjusting to the conditions of British life, feel themselves to be a group apart, different somehow from their fellow citizens, both Jewish and Gentile. But she does not show how their perceptions, values, and sentiments differ significantly from those of Britons of similar rank and education. All we are told is that her respondents feel that they are different; ethnicity, surely, has more content than this.

Beyond the question of Berghahn's success in translating theory into practice, there is the more fundamental matter of the relevance of assimilation in Germany, which provides the starting point for this study, to understanding refugee adjustment in Britain. It could be argued that the two situations were rather different—after all, in Germany the Jews were citizens and native speakers, more or less at home in the majority culture, while in Britain they clearly were not—and that Berghahn would have been better served by drawing terms and concepts from the literature of immigrant adjustment in general and Jewish migration in particular. Indeed, it is curious that she fails to illuminate her subject by exploring comparisons with the migration of German Jews to England and America in the previous century or with the settlement of German refugees from

Nazism in the United States, of which there is now an excellent account by Steven Lowenstein.

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AMBROSE MACAULAY. *Patrick Dorrian: Bishop of Down and Connor, 1865–85*. Dublin: Irish Academic Press. 1987. Pp. 397. \$55.00.

When Patrick Dorrian was born in 1814, Irish Catholicism was relatively easygoing in its ecclesiastical discipline and devotional life. Its hierarchy was dominated by men who sought to further the interests of their flock through tolerant accommodation with Protestantism and liberalism and by reassuring the authorities of their loyalty to the British connection. By the time Dorrian died in 1885, Catholic religious practice had been regularized and strict discipline imposed on clergy and laity. A triumphalist church had set its face against the liberal nostrum of mixed education, and its leaders aggressively defended their community's interests against any vestige of discrimination in favor of the late established church. Moreover, they had associated their institution with the rising tide of Irish nationalism.

Dorrian's appointment in 1860 as coadjutor with right of succession to the bishop whose diocese included Belfast was a clear step in the direction of all of these changes in the Irish Catholic church. He succeeded a prelate, Cornelius Denvir, who had dragged his feet as the hierarchy had moved decisively away from its earlier tolerant attitude toward mixed education and who had been cautious—his opponents said timid—in erecting churches and other facilities to accommodate the growing Catholic population of Belfast. Dorrian's episcopate was marked by ambitious construction of churches and schools and consequent demands on the laity for funds as well as efforts to achieve clerical dominance in the life of the Catholic community in Belfast.

Ambrose Macaulay tells this story clearly and effectively. The archival and printed sources are very rich indeed, but the author is always careful to place the details of Dorrian's career in the context of larger issues in Irish history. Macaulay is perceptive and critical in evaluating the shortcomings as well as the achievements of his subjects.

In addition to the momentous changes in Irish Catholicism noted above, the city of Belfast underwent dramatic transformation during Dorrian's lifetime. In 1814, Belfast was a Presbyterian mercantile center of perhaps thirty thousand residents, whose liberal elite had only just repented their incautious alliance with Catholics in the radicalism of the 1790s. By 1885, Belfast was a major

industrial city of about two hundred twenty-five thousand inhabitants, politically dominated by Conservatism and including both a Protestant and a Catholic working class who engaged in chronic violent conflict.

Macauley also deals with these changes, and he is fair-minded in pointing out, for example, that Catholics as well as Protestants were capable of needlessly provocative behavior. Disappointingly, however, he passes up the opportunity to reflect on possible connections between these melancholy changes in Belfast, on the one hand, and the concurrent changes in the Catholic church—and, indeed, the Protestant churches as well—on the other.

DAVID W. MILLER
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CATHERINE B. SHANNON. *Arthur J. Balfour and Ireland, 1874–1922*. Washington, D.C.: Catholic University of America Press. 1988. Pp. xvi, 362. \$38.95.

The role of Arthur J. Balfour in the shaping of modern Ireland has largely been understated. At least a dozen English and Irish politicians have received more attention from historians seeking to understand the ferment that engulfed that country in the nationalist era. Catherine B. Shannon's excellently crafted study helps to rectify that inadequacy without lapsing into overstatement. The author integrates the familiar story of Ireland's awakening with the multifarious activities of a man who occupied many of the highest posts in the British government. Few of the details are new or surprising, but their synthesis should stimulate some renewed thought on Anglo-Irish relations.

As chief secretary of Ireland from 1887 to 1891, Balfour instigated a law-and-order program that paved the way for social and economic reforms that he hoped would "kill Home Rule with kindness" and stop socialism before it began. Shannon deftly describes how Balfour and his successors, Gerald Balfour and George Wyndham, implemented his philosophy of constructive unionism through land purchase schemes and local government reform as well as legislation aimed at improving the agriculture, trade, railways, education, and fiscal condition of Ireland. Such laudable endeavors, however, were offset by Balfour's national and class prejudices that blinded him to the political realities of Irish life. "Balfour measured everything by an Anglo-Saxon yardstick" (p. 70). Indeed, the very success of his policies in encouraging greater control by the Irish over their economic and social destiny, Shannon argues, only whetted their appetites for political self-determi-

nation. Ultimately, Balfour's amelioration policy was wrecked by the devolution crisis of 1904–05.

Shannon shows that, in the Unionist backlash that followed, Balfour deserves greater responsibility for his party's intransigence over Ireland than has generally been assumed. In her analysis of the ensuing "Dangerfield era" and the war years, she employs the well-worn grooves established by other historians as a context for displaying Balfour's largely negative influence. His famous pronouncement in 1910, for instance, that he could not become another Robert Peel to his party is followed by an analysis of the factors that led him to reject the federalist and coalition proposals being considered by his party. She also relates with telling effect how the uncompromising position of Balfour over Ireland served the ulterior purpose of unifying the Unionists and foiling the Liberals. Furthermore, although he hoped that Ulster would be the primary stumbling block to Home Rule, his resort to the "Orange card" virtually ensured partition in the event that his party was ever forced to concede the principle of Irish self-government. With the failure of various conciliatory ventures after the Easter Rebellion of 1916, this kind of solution appeared more likely. Only in her discussion of events from the Cabinet committee report in 1919 to the promulgation of the Government of Ireland Act of 1920 is there a strain on the evidence; Shannon's narrative is suddenly peppered with equivocating expressions in her attempt to establish something more than a circumstantial link between Balfour's "two-nation theory" and David Lloyd George's Irish policy. But her more general conclusion that Lloyd George could hardly ignore the views of the Conservatives' oldest and most distinguished leader is quite sustainable.

The foremost quality of this account is its sense of proportion. Notwithstanding the likely predispositions of an Irish-American historian, one detects a note of empathy with her subject, a quality that goes a long way to establish credibility. This study should inspire other scholars of Ireland to explore the fertile ground of Anglo-Irish relations from the Unionist side.

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LOUIS PEROUAS *et al.* *Leonard, Marie, Jean et les autres: Les prénoms en Limousin depuis un millénaire.* Foreword by JEAN DELUMEAU. Paris: Centre National de la Recherche Scientifique. 1984. Pp. x, 229. 170 fr.

What's in a name? Lots, according to Louis Perouas and his fifty-person research team that, under the auspices of the Centre National de la Recherche Scientifique, drew from medieval chartularies, tax rolls, *livres de raison*, parish registers, and *état civil* records a staggering sample of almost a million first names bestowed on babies in the Limousin from Merovingian times until 1940. Over this truly *longue durée*, Perouas and his colleagues found distinctive patterns in naming that, they argue, reflect major currents of religious, social, cultural, and even political change.

Most striking of these patterns is the slow formation of what the authors call the "Christian model" of naming, which began to take shape in the thirteenth century and reached its apogee during the Counter Reformation, only to disintegrate after the French revolution. In this model, names were bestowed under church control, at baptism. Babies were usually named after one of their godparents, and their names were chosen from a very restricted repertoire, consisting mostly of names of the Holy Family and Christian saints and martyrs. A name did not express its bearer's individuality, as it does today. Instead, a name's function was integrative. Given the small repertoire, most children shared their name with others in their village and even in their family. Children's names symbolized their membership in a family (godparents were usually family members, and, therefore, family names were passed down over generations), a locality (children were often named after locally famous saints, like Léonard and Martial in the Limousin), and the community of Christians (the saints whose names children bore became their patrons and role models).

This "Christian model" of naming began to disintegrate with the spread of dechristianization in the late eighteenth century. Even before that, nobles and bourgeois, perhaps to mark the individuality of their children or to set themselves apart from *le peuple*, had enlarged the standard repertoire by giving their children multiple names and borrowing new names from classical antiquity. The breakdown in naming traditions accelerated momentarily during the revolution, with its attempt to replace religious names with secular ones, and really gathered momentum in the mid-nineteenth century, when migrants brought back from Paris exotic, romantic names derived from popular literature.

This book makes a convincing case for names as barometers of major historical changes, although, as always with products of the *Annales* school, American readers might wish for more sophisticated quantitative methods: at least tables with N's, if not regression analyses. As this book shows,

naming patterns can be used to trace such phenomena as the spread of the Counter Reformation, the divorce between elite and popular cultures, the depth of commitment to the revolution, and the impact of migration on the nineteenth-century countryside. Names are especially revealing about attitudes toward gender. For example, the tendency to give more boys than girls revolutionary names in the 1790s probably reflected the revolution's denial of political roles to women. Clearly, names will henceforth be a useful if not major weapon in the historian's arsenal.

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NATALIE ZEMON DAVIS. *Fiction in the Archives: Pardon Tales and Their Tellers in Sixteenth-Century France*. Stanford: Stanford University Press. 1987. Pp. ix, 217. \$22.50.

This unusual book owes its originality and its distinction to the wide range of the author's historical imagination, that is, finding linkages that others have overlooked, combining sociological, political, and legal aspects of history with literary analysis and psychological insights. The resulting mix makes significant contributions, multi- and interdisciplinary. Owing to the high drama of the pardon tales—some tragic, some funny, almost all suspenseful, as in detective stories—a variety of readers, impatient to know “how it comes out,” will enjoy a “good read” along with new material and fresh interpretations.

The juxtaposition and cross-fertilization of different kinds of sources is the foundation of the innovative methodology. Natalie Zemon Davis is concerned with the “shaping, molding elements in the crafting of a narrative,” specifically in “defining the character of historical narrative” (p. 3). Distinguishing “historical time” as a framework (wars, royal policies, social crises, and so forth) that makes the historian “itch to use it as an illustrative document in a graduate seminar” (p. 56), she demonstrates the prevalence in the tales of “ritual and festive time.” A man who killed his adulterous wife on the day of the Magdalene, in thinking about it later, concluded that “the Magdalene's feast was the appropriate time to decide not to die for a whore and not to let her live. This way of thinking about connection takes some sustenance from medieval and Renaissance notions of correspondences and analogies between the supernatural and the natural world. . . . Here we have a dynamic convergence between ritual programs and ceremonial scenarios on the one hand and particular life events on the other. . . . The story-

teller uses the ritual or festive frame . . . to make sense of what has happened” (p. 29).

Letters of remission are “many-layered” (p. 20) sociologically, in that the selection of 164 male pardons and 42 female is drawn from a cross section of the population and from all parts of France. (See figures for the categories in the note on page 18.) They are also “many-layered” culturally: “Information, values and language habits could flow across lines of class and culture. These stories . . . were spread by notaries, clerks, chancery officials, attorneys, judges . . . sometimes by the king himself. . . . We have here not an impermeable ‘official culture’ imposing its criteria on ‘popular culture,’ but cultural exchange, conducted under the king's rules. The stakes were different for supplicants, listeners and pardoners, but they were all implicated in a common discourse” (p. 112). Among the intriguing linkages is the correspondence of many pardon tale themes with those of Boccaccio, Rabelais, and Marguerite de Navarre.

Probably least familiar to most readers is Davis's careful explanation, analysis, and illustration of the complex processes of justice in sixteenth-century France. There were thirteen “extenuating circumstances for homicide” (p. 12 n.) and a considerable number of middlemen involved. Motivation is central to the “credibility” of the tale; much also depended on the status and influence of the advocates who brought it to the attention of the king, who alone had the power of pardon, acting through a judicial body, often the Parlement of Paris itself. Yet “even the monarch has an interest in the prisoner's voice being the dominant one. . . . Collaborative product though it was, the letter of remission can still be analyzed in terms of the life and values of the person saving his neck by a story” (p. 25). Authors and readers seem to have agreed that this be presented as “news”; dealing with real events, the *vraisemblance* of a pardon tale was increasingly seen as an element in its credibility, in contrast to the fantasies of chivalric romance. Davis's book contributes a “sociology of justice” to the early modern field.

Davis brings out a fresh angle of the familiar theme of the growing power of the crown. A large percentage of the petitions succeeded. “Of all the prisoners in the Paris Conciergerie from 1564 (when the record begins) to 1580, only 6.5% were deprived of the king's grace” (p. 51). This success goes beyond the effectiveness of the story, “as the supplicant was integrated into the larger drama of the build-up of monarchical power” (p. 52). Intermediary authorities had gradually been deprived of the right to pardon. “From the Ordinance of Blois, 1499, through the *Republic* of Jean Bodin, pardon was celebrated as ‘one of the fairest marks

of sovereignty,” and “the king’s special pardons when he entered a town for the first time after his coronation were as important as his touching for scrofula” (pp. 52–53). Nevertheless, it is essential to remember that at all stages this process required “playing by the king’s rules.” “The habit of language insisted upon in the letters of remission and the roles in which supplicants were required to present themselves were among the civilizing mechanisms of the early modern French state . . . reminding people of the locus of power” (p. 57).

NANCY L. ROELKER
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HILTON L. ROOT. *Peasants and King in Burgundy: Agrarian Foundations of French Absolutism*. (California Series on Social Choice and Political Economy, number 9.) Berkeley and Los Angeles: University of California Press. 1987. Pp. xi, 277. \$35.00.

Early modern France has long fascinated historians interested in the development of capitalism as well as the relationship between twenty million Frenchmen and a monarchy determined to consolidate and centralize its powers. What factors gave impetus to this consolidation? How did the peasants and their village communities fit into the new absolutism? Was it necessary to destroy the traditional village community in order to create this new monarchy? Or were these villages, with their tight organization and centuries-old traditions, important allies in the struggle to suppress the feudal aristocracy? What role did the king’s representative on the scene—the intendant—play in the consolidation? How—and why—was the village protected from the forces of modernization in the eighteenth century? How does this process compare to the experiences of similar communities across the Channel and to the east? Finally, what was the effect of the revolution on the village community?

Hilton L. Root addresses these questions in this book. In one sense, Root’s study is broader than the title indicates because he examines trends of centralization and modernization common to all of France and compares village communities in Burgundy to those in England, Prussia, Russia, and elsewhere in Europe. In another sense, however, the book is more narrowly focused than its title indicates because the author restricts his study to the grain-producing, open-field system that existed in the northeastern section of Burgundy—the Côte d’Or—and does not include the Burgundian areas of viticulture or cattle raising.

The intendants and their subdelegates, of

whom there were thirty-four in Burgundy by the end of the *ancien régime*, left a rich correspondence that Root has used. “The intendants were above all fiscal agents; they reported to the *contrôleur général*” (p. 219). These officials considered their primary task to be the collection of local taxes, which they delivered to the royal treasury. The economic well-being of the village community was thus of paramount importance since solvency on the village level ensured prompt local tax payments and avoided the social problems attendant on poverty. Any individual or institution—reform-minded royal official, parlement, estate, court, or seigneur—was a potential enemy. Anything or anyone that impeded the province’s ability to pay its annual taxes had to be confronted.

Solvent village communities were a necessity if funds were to be raised to fight the wars of Louis XIV and his successors. Because royal administrators “had to operate with a limited number of personnel, they concluded that enforcing collective liability was the most efficient way to collect taxes” (p. 13). Seigneurial authority had eroded as the lords were removed from the supervision of the village assembly. Royal officials simply adopted the responsibility of the collective village tax, formerly insisted on by the feudal lords. Root observes that “in its movement toward absolutism, the monarchy interposed its justice, its taxes, and its bureaucracy between lord and peasant” (p. 65).

The crown recognized that it would be far easier to deal with the village community as an entity than it would have been to control individuals or groups. The intendants did not want to create a local council of notables that would have been confined to the wealthier and therefore more independent peasants. So they insisted that all heads of household attend meetings of the village assembly and participate in the decision-making process. Full participation in the village assembly had the advantage of binding the minority to the “general will.” The wealthier minority of peasants saw councils of notables both as assertions of their independence and as instruments to curtail absolutism.

By the second half of the eighteenth century, the *physiocrats* and others generally agreed that French agriculture had to be freed from its medieval restraints. The technological progress that was occurring in England, which was so obvious to agronomists, could not be realized in France until the system of “common-use rights and properties” (p. 105) was eliminated. The list of needed reforms was a long one: lifting restrictions on the grain trade, ending the equal distribution of communal commodities such as wood, partitioning common lands, encouraging enclosure, eliminat-

ing the practice of common pasture. These changes would undoubtedly have increased agricultural output. They would also have brought vast rural dislocation, to which large numbers of small holders objected, remaining "committed to the continuation of open fields, mandatory fallow land, and communal pastures" (p. 153). Fearing that, at least in the short term, these policies would impair the villages' ability to pay their royal taxes and also contribute to rural poverty, the Burgundian intendants opposed such progressive changes.

Root points out that attacks on seigneurial rights and privileges increased in the years just before the revolution. Not only were the obligations being assailed but, in the Age of Enlightenment, more and more questions were being raised about the historical justification of these rights. The growth of the royal bureaucracy, the end of the seigneurs' leadership role in the village, and the development of the legal profession resulted in a profusion of Burgundian legal cases in which seigneurial rights and privileges were placed on the defensive.

After 1750 the government attempted to implement agricultural reforms and free the economy by issuing a series of royal edicts. But these efforts were thwarted by successful earlier royal policies to preserve communal rights and properties. The Burgundian intendants, as fiscal agents of the *contrôleur général*, continued to focus on their primary objective—meeting Burgundy's annual tax bill. The obstructionism of the intendants combined with an increasingly precarious fiscal situation and the absence of English-style parliamentary enclosure bills explains why agricultural production remained static at the end of the *ancien régime*. With its attacks on "feudal hindrances" including guild regulations, the rhetoric of 1789 was designed to free the French economy from artificial restrictions. But Root observes that by 1793 revolutionary officials had learned the lesson of their predecessors: "village solvency [and thus ability to pay taxes] depended on the preservation of communal properties" (p. 239). Once again, questions of village debt, tax collection, and the needs of the cities triumphed over economic theory.

Root has made an impressive contribution to the study of the development of absolutism, early capitalism, and peasant conservatism. His work will also be of value to those interested in village organization and the growth of the intendancy as an instrument of state development. One minor caveat: despite the fact that this study is centered on Burgundy, the reader will look in vain for a map to place that province in the context of

France or Europe or a detailed map of Burgundy to help identify its regions and villages.

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LOUIS MARIN. *Portrait of the King*. (Theory and History of Literature, number 57.) Translated by MARTHA M. HOULE. Foreword by TOM CONLEY. Minneapolis: University of Minnesota Press. 1988. Pp. xvii, 288. Cloth \$35.00, paper \$14.95.

This book is a brilliant set of meditations on readings that serve to illustrate the functioning of absolutism in the reign of Louis XIV. Louis Marin takes as his point of departure Ernst H. Kantorowicz's *The King's Two Bodies* (1957), as does Jean-Marie Apostolides in his *Le Roi-machine: Spectacle et politique au temps de Louis XIV* (published, like Marin's book, by Minuit in 1981). Apostolides's argument is diachronic: when the monarchy began to disintegrate, the bourgeoisie insinuated itself into the king's newly vacant "imaginary" body. The book under review is a fundamentally synchronic study of the semiotics of power.

Marin's thesis is based on the rhetorical figure of chiasmus: in Louis XIV's kingdom, representation was power, and power was representation. Only in the last part of the book does the author explicitly ask the ontological questions underlying this chiasmus: Was the king anything more than his self-representation or "portrait?" Was there any essential power behind the dazzling facade of the Sun King's continual spectacle? Tom Conley suggests in his introduction that this discussion applies to power in general. But, although Marin raises issues that remain disturbingly relevant to the twentieth century, Conley's conflation of seventeenth-century spectacle and twentieth-century media is, I believe, unwarranted by Marin's historically grounded study.

As if to demonstrate the politics of representation, Marin constructs his book as a court ballet, complete with an overture, finale, three entrances, and two interludes. The three entrances correspond to what he identifies as the king's three bodies: the physically historical, the juridical, and the sacramental. The sacramental body mediates the other two in a unity composed of all three. The entrances are formed by readings of texts as diverse as Paul Pellisson's proposal (written around 1670) for a history of Louis XIV, the Petite Académie's "metallic history" of Louis's reign (1702), Jacques Gomboust's map of Paris (1652), and two texts by André Félibien, a guide book of Versailles (1674) and a description of the six-day royal festival held at the château in 1674. The analyses in the entrances offer stunning ex-

amples of what Marin, following Pascal, calls the "effects" of absolutist representation. Marin shows how histories of the Sun King created a play of light and mirrors in which all subjects bathed in his splendor, how royal coins and medals gave currency to the king's power and established him as the founder of exchange value, how royal festivals functioned as miraculous signs of Louis's reign.

Marin calls the texts of the interludes parodic, because in exposing the mechanisms of absolutist representation they serve to deconstruct the elaborate semiosis of power deployed in the entrances. These texts include a fable by La Fontaine, a speech by Jean Racine at the Académie Française, and a fairy tale by Charles Perrault.

Fittingly, excerpts from Pascal open and close the ballet. Pascal's *pensées* about force and justice lead Marin in his overture to describe how a discourse of force coopts "justice" when the state acquires enough "signifying surplus-value" to overwhelm its people with a show of pomp. The finale answers the ontological questions in the negative: behind the king's portrait there was nothing more than a man. Marin uses Pascal's "dispossessed king," an image of humanity fallen from grace, to reveal that royal representation was a tragic parody of the eucharist. There was no real presence in the king's imaginary body.

Martha M. Houle has made good sense of a difficult book in a translation that is perhaps necessarily somewhat literal. This study is well worth the effort it demands of the reader. It offers a complex and inimitable analysis of the culture of absolutism.

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JAMES B. COLLINS. *Fiscal Limits of Absolutism: Direct Taxation in Early Seventeenth-Century France*. Berkeley and Los Angeles: University of California Press, 1988. Pp. xii, 256.

Is the study of royal finances becoming the preserve of English-speaking historians of early modern France? During the last fifteen years there have been at least five books in English devoted to the subject, while several others have paid it significant attention. French-speaking historians, on the other hand, do not seem particularly interested.

In this context James B. Collins's book is frustrating. Whereas Richard Bonney in *The King's Debts: Finance and Politics in France, 1589-1661* (1981) generally chose to acknowledge his co-workers by attacking them, Collins tends to ignore their existence. Frustration also comes from Col-

lins's unfulfilled promises to discuss various matters fully.

Chapter 1 is a survey of the development of the direct taxation system in France between 1300 and 1598. Chapter 2 is a description of the attempts to reform direct taxation between 1598 and 1648. Collins calls the action of the government in 1634, which transformed rights to surtaxes into annuities, a partial bankruptcy at the expense of office-holders who, as a consequence, were alienated. This alienation is said by Collins to have forced Louis XIII to change the nature of his government through the introduction of intendants. It was also "a major factor in the widespread popular uprisings of the period 1624-48" (pp. 98-99). Unfortunately, adequate proof for these statements is not provided.

Chapter 3 is a discussion of revenues from direct taxation during the first half of the seventeenth century (though the figures available after 1636 are suspect). The main contribution of this chapter is an explanation of the reasons why the figures concerning direct taxation revenues in Jean-Rolland Mallet's *Comptes rendus de l'administration de la France . . .* are inaccurate. Because Mallet's book is the standard source for information on the income and expense of the early modern French government, Collins's critique is important. Collins explained his argument, however, in an article published in *Annales* in 1979. Moreover, when all of Mallet's figures are used (indirect taxes, *parties casuelles*, extraordinary income), the pattern of taxation rates is essentially the same as the one Collins develops.

In the fourth and final chapter, which concerns the provinces, Collins is most at home. Here he builds on his earlier work and provides a broad study of taxation at the local level. Particularly useful are his comments on women, locally itinerant peasants, and the complexities of local taxation.

When one looks back over the work of English-speaking historians of royal finances during the past decade and a half, one finds many points of agreement. The French kings and their advisers were working to build a stronger central government. There was opposition and support at the local level, depending on individual self-interest that was both personal and tied to social position. The government tended to favor quick money at the expense of substantial losses in revenue. The bourgeoisie was important in tax collection and tax payment though not because of direct taxation. The peasants suffered the most, and by the 1630s the level of their taxation was literally unbearable.

Collins has provided some important information, especially on politics and finance at the local

level. Work remains to be done on the 1640s and 1650s, in particular. Work by others that ties the history of prices to government income and expense has not been followed up. Above all, someone needs to consider the findings of all historians of the French government's taxation policies in the light of recent work by early modern French historians.

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PAUL J. MORMAN. *Noël Aubert de Versé: A Study in the Concept of Toleration*. (Texts and Studies in Religion, number 32.) Lewiston, N.Y.: Edwin Mellen. 1987. Pp. 282. \$49.95.

One valuable approach in intellectual history is the study of minor thinkers in an attempt to understand better the climate of opinion of an age. This is what Paul J. Morman has done in his biography of Noël Aubert de Versé (1645–1714), a French controversialist involved in the important issues of the 1680s. Aubert seemed to have his hands in the great matters of public debate, taking on Spinoza, the Cartesians, Bossuet, Jurieu, and Louis XIV.

There is some consistency in Aubert's beliefs, but one would not know this from his professions of faith. Born a Catholic in Le Mans, in 1662 he converted to Calvinism, apparently out of conviction. Defrocked as a minister, primarily owing to a too liberal theology, Aubert returned to the Roman church in 1670. A pension might have made the transition attractive, though Aubert did believe by this time that the Gallican church was best able to reunite Christendom. Scandal seemed to dog Aubert, and in 1679 he fled to the United Provinces. The revocation of the Edict of Nantes dashed his hopes that the French church would unify Christianity, so he became Protestant once more. Alas, he had little luck finding a secure position in either the United Provinces or in Germany; in 1690 he returned to France, reconverted again, and soon obtained a pension. This religious odyssey raises questions about the nature of seventeenth-century conversions, but it does not appear to have done much damage to Aubert's psyche. Morman points out that Aubert believed almost any Christian denomination could lead one to salvation. Unlike the overwhelming majority of his coreligionists, Catholic and Calvinist, Aubert did not consider membership in any particular sect to be crucial. Rather, he reduced the essence of Christianity to charity and brotherly love, finding them in the Apostles' Creed, the Ten Commandments, and the Lord's Prayer. On this foundation, Aubert constructed a theory of toleration that found new admirers in his era.

Aubert never doubted the truth of Christianity; this irenicist believed that the unfettered use of reason and the dismissal of unessential dogmas would bring Christians together. In fact, it was Christianity that concerned him more than toleration, for he did not grant toleration to atheists. He allowed the individual the right to follow a free and erring conscience, but only so far. Reason was to buttress faith, even varying ones, but not to go against all faiths.

Aubert distinguished between civil toleration, the government's obligation to tolerate different religions as long as they did not threaten the state, and ecclesiastical tolerance, the need for Christians to tolerate all who accepted the fundamentals of Christianity, even if they disagreed on peripheral religious beliefs. Aubert had a problem with civil toleration, because the state had to decide when a religion became dangerous. The example of the revocation illustrated the weakness of a theory of toleration that relied on a sovereign's right to determine whether a religion posed a threat. But Morman is careful to present Aubert's limitations and significance; he avoids the pitfall of some biographers who claim too much for their subjects. Thus, Morman explains that Aubert anticipated but did not influence the philosophes with his critical reasoning, anticlericalism, and theory of toleration. Aubert was representative of a group of prolific controversialists in a premier age of controversy, one mired in conflict among different faiths and between faith and reason. He was far beneath Bayle, Jurieu, Arnauld, Bossuet, and Nicole but worthy of a full-length study. Morman's monograph will interest specialists in the decade of the 1680s.

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ROGER CHARTIER. *The Cultural Uses of Print in Early Modern France*. Translated by LYDIA G. COCHRANE. Princeton: Princeton University Press. 1987. Pp. xi, 354. \$35.00.

For some time specialists in the history of Old Regime France have delighted in the many monographs and articles flowing from the very prolific Roger Chartier. This author has published widely on education, the history of the book, the history of political ideas, cultural history, and other topics.

Yet, in the United States, Chartier's work has received far less attention than it deserves. Many of his essays have been almost impossible to find on this side of the Atlantic. And almost none of his work has been accessible to those who do not read French. Consequently, this translation of eight articles will introduce the considerable abilities of

Roger Chartier to many. Furthermore, because he has researched in several different schools of history whose techniques are not yet well known here, this selection will make available to North Americans a variety of new approaches.

The material presented in this volume, though not representative of all aspects of Chartier's work, does provide a fair selection of his research on the history of ideas. Among the articles are three that trace concepts over a long period. The subjects include transformations of festivals, of the art of dying, and of the term *civilité*. These articles reveal Chartier's interest in an intellectual history that concentrates on widely held beliefs instead of the perceptions of intellectuals. His selection of topics is innovative, borrowed in part from a new wave of research. And, in examining these social views from the eighteenth century, Chartier remains very sensitive throughout the volume to the way that different groups and individuals held various understandings of similar situations. Such a perspective enriches Chartier's analysis.

Four of the articles in this book explore the reading habits of the French in the early modern era. Chartier examines publishing strategies, contents of popular works, and the possibilities for the circulation and consumption of books in cities. Although these articles do not cohere exactly, some very useful generalizations do emerge. Chartier shares the view of many scholars that a wide spectrum of the population was acquainted with those works that historians originally believed were designed only for the poor. Yet, if social cleavages seem to diminish in this work, gaps between city and country seem to yawn ever wider. The growing distribution of books apparently increased the urban reader's advantage.

One other article concerns the grievances compiled by the French in 1789 as part of the electoral process for the Estates General. This piece very carefully and cautiously mines these documents for the state of public opinion on the eve of the revolution. Throughout the volume but especially in this work, Chartier skillfully weaves original research with an extraordinary mastery of the scholarship of others. This technique leads to fascinating conclusions, particularly regarding the rapid radicalization of opinion in the closing months of the Old Regime. This article also emphasizes the relationship between elites and peasants and thus is somewhat linked to his other articles that address social divisions. The connections between any of the pieces, however, are on the whole rather limited. Indeed, the strength of the volume is not found in any overall substantive conclusions but in the discovery and presentation

of Roger Chartier's talent to an American audience.

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L. W. B. BROCKLISS. *French Higher Education in the Seventeenth and Eighteenth Centuries: A Cultural History*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xiii, 544. \$92.00.

In a work of masterful synthesis and breadth, L. W. B. Brockliss examines teaching and student life in early modern French *collèges* and universities, a subject with intellectual, class, and political implications. Brockliss is attentive to nuance in analyzing the content of courses that developed over a period of two hundred years; he treats all of France, provincial and Parisian. As a result, this study is a complex and subtle history of ideas.

Brockliss argues that, although educational institutions were coopted in seventeenth-century France by the political and religious power structures, universities unintentionally prepared students for the Enlightenment by exposing them to intellectual controversy. He both confirms and challenges standard opinions about the early modern educational establishment. Professors did indeed teach received knowledge rather than develop new ideas in the classroom. In general, the courses in theology, law, and moral philosophy tried to conform to the interests of church and state by inculcating students with traditional and absolutist precepts. Interestingly, law students tended more than others to be truant and rowdy, and Brockliss hypothesizes that such low morale reflected their conviction that the law course was "largely pointless" (p. 281). But many professors did take account of new ideas, sometimes defending them, more often including them in eclectic theories or arguing against them. Cartesianism was incorporated systematically into the two-year course in philosophy that the *collèges* offered after 1670. Such exposure was important, but a still more decisive influence came from the teaching of the sciences.

Beginning in the 1670s professors in physics, mathematics, and medicine reflected recent developments in their teaching. Additionally, physics, which all university students studied, introduced the principles of reason, utility, and empiricism and encouraged an appreciation for innovation and argument. In Brockliss's view, French higher education was crucial to the acceptance and then rejection of absolutist political theory, to the dissemination of methods and controversies of the scientific revolution, and ultimately to the open-

ness of the educated classes to writings of the philosophes.

Brockliss's account is never simplistic but balances intellectual trends against contemporary issues and educational realities. Teaching developed within the context of parliamentary prerogatives, theological disputes about grace, ecclesiological conflicts over Gallicanism and ultramontanism, and debates between Aristotelians and Cartesians or among Galenists, "fluidists," and "solidists." These political, religious, and scientific influences on early modern education are skillfully portrayed.

Whether or not future scholars confirm his circumstantial argument that higher education indirectly supported the Enlightenment, Brockliss's synthesis of the intellectual currents affecting *collèges* and universities is a considerable achievement. Plentiful and varied archival sources and obscure printed materials, including financial and legal documents, theses, and professorial polemical literature, permit Brockliss to see beyond the regulations into the reality of education. As a result, he can explain the reasons for educational policies and changes, and he is sensitive to the views of parents, students, teachers, and the larger community.

This book is our most comprehensive account of higher education in seventeenth- and eighteenth-century France. It is not exhaustive but provides a context for future studies of individual cases (institutions, professors, students, courses, and textbooks) or subjects outside the scope of the present work (for example, incomes of professors, technical education, education of women, class issues). Eight maps illustrate the distribution of French educational institutions, and the prosopographical appendix is a helpful starting point for research on the French professoriate. There are hints throughout the book of topics, especially in the realm of provincial learning (Caen, for example, seems to have been an educational pioneer), meriting further study.

Unfortunately, the book is an unreliable guide to sources for future researchers because of carelessness in the citation of manuscripts and the rendering of names (for example, Yves Laissus, François Lebrun, Maarten Ultee, and Joseph Piton de Tournefort). The French word *exercice* is anglicized throughout, and typographical errors abound. The index conceives subject listings too narrowly, subject and name entries are not exhaustive, and page references are sometimes inaccurate. Pierre Varignon joined the Académie Royale des Sciences in 1688, not 1699.

Despite such flaws, the book is an important contribution to intellectual history and a suggestive model for future research. Brockliss weaves a

rich tapestry of seventeenth- and eighteenth-century thought, both within educational circles and outside them. He is sensitive to changes over time and to differing points of view at the same time; he is fair to both majority and minority opinions and succeeds in conveying the intellectual pluralism of this period, taking no single school of thought, no individual, as representative. Brockliss has achieved an impressive synthesis of unruly material and in so doing has provided a revealing picture of the intellectual trends and controversies familiar to the educated classes of prerevolutionary France.

ALICE STROUP
Bard College

THOMAS BRENNAN. *Public Drinking and Popular Culture in Eighteenth-Century Paris*. Princeton: Princeton University Press. 1988. Pp. xiv, 333. \$38.00.

In this excellent study, Thomas Brennan takes us into the tavern, the worker's second home and a realm little scrutinized by historians. As the title indicates, Brennan's subject is not just drinking but also culture, the culture "structured" and "revealed" by tavern conviviality (p. 12). Brennan argues that the Parisian cabaret was neither "disreputable" nor "marginal" but "an important part of popular culture." Few historians would challenge this notion, but Brennan's work is nonetheless welcome.

If historians have neglected the tavern, it is probably because the subject poses serious documentary problems. Brennan tackles with aplomb one of the few appropriate sources, the voluminous records of the Parisian Châtelet commissioners, and he supplements those records for the years between 1691 and 1771 with death inventories, guild records, and literary sources from other Parisian archives. Brennan demonstrates that Paris housed about three thousand taverns in the mid-eighteenth century that provided workers, from master artisans to day laborers, with space for conviviality. In public drinking establishments, workers reinforced affinities based on work, especially on the guilds, and strengthened ties based on neighborhood or quarter. Workers also came into conflict in the tavern, and Brennan uses the reports of rowdy behavior to uncover the "rules" of public drinking and describe the limits of working-class "honor." He also includes a chapter on the purveyance of drink and the policing of taverns.

Like many historians of late, Brennan stresses the differences between elite and popular culture. He points out that the elites abandoned the neighborhood tavern in favor of the newer café, and

public drinking became an elite metaphor for popular disorder. At the same time, popular drinking habits changed, too. The working people began to frequent *guinguettes*, large suburban drinking establishments that provided cheap wine and especially, a sense of freedom and escape. Women frequented these establishments unlike the urban tavern that attracted women's ire and condemnation. Brennan provides a particularly sensitive explication of feminine opposition to masculine sociability.

One does wish that the *guinguette* had been treated at greater length, that changes in popular drinking habits and culture had been given more attention, and that the chronological scope of the book extended to the years before the revolution. The "values" that Brennan claims tavern behavior illuminates might also have received more attention. We learn that workers prized their conviviality and the drink that cemented it. But what other values did they harbor? Did their notion of honor differ from that of the elites? Did a different concept of work and morality emerge in talk around the wine bottle? Perhaps such insights are impossible to obtain. We cannot listen at the workers' side nor eavesdrop on their cabaret conversation. Brennan's study offers us the next best thing: an illuminating view of one important aspect of the Parisian workingman's social world.

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MICHAEL SONENSCHER. *The Hatters of Eighteenth-Century France*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiv, 187. \$28.00.

Michael Sonenscher's analysis of why eighteenth-century French hatters refused to make more than two hats a day, even when the substitution of rabbit for beaver fur meant they could easily have produced three, proceeds on two levels. On the one hand, it is an analysis of changing work processes and master-journeyman relationships. On the other, it is an analysis of changing conceptions of property, labor, and jurisprudence in eighteenth-century France. It is the second level of analysis that particularly interests Sonenscher.

On the first level, Sonenscher analyzes the core-periphery nature of the hatting craft, describes the process by which hatters turned animal pelts into felt hats, and chronicles the mid-century conflicts that surfaced between journeymen and master hatters as the Atlantic trade in beaver pelts dried up and previously despised rabbit skins were substituted for beaver pelts. Unfortunately, for the women who removed the fur from the pelts

and for the men who turned the fur into felt, the substitution of hare for beaver led to the soaking of pelts in a lethal solution of water, nitric acid, and mercury. Using this solution made it possible for felters to produce three rather than two hats a day—a development they resisted, Sonenscher argues, because of their notions of property.

The payment of hatters by the piece, not by the day, ensued from what Sonenscher regards as a legal fiction: hatters owned the hats they produced. This notion made it impossible for felters to cooperate in the production of a hat, even though they worked in pairs, sharing tools and work space, and made them fear that, if they agreed to make three hats a day (hence, to work for lower piece rates), the tasks they performed for which they technically were not paid (unloading and moving timber and charcoal, mixing solutions, lighting fires, collecting materials) would be inequitably divided and some of them would lose income. Sonenscher uses the hatters' conceptions of labor and property, and the growing tension between masters (who wanted to increase production) and journeymen (who wanted to maintain production norms initially established by the masters), to discuss the meaning of work, the origins of law, and the impact of political theories (for example, the nature of property) on production processes.

Sonenscher's analysis of these issues is an interesting new approach to understanding the complexity of artisan "culture." I am not entirely convinced by parts of his analysis, however. I wonder if what he calls the "unpaid work" of hatters might not have been regarded as part of the process of making hats and, therefore, in fact, as paid work. I am not persuaded that "the best way to understand what happened when hats were made in eighteenth-century France is to read Grotius, Pufendorf, Barbeyrac, and Burlamaqui" (p. 2). I think that work processes reveal at least as much as political theory does about the customs and *mentalités* of artisans, and I wish the book contained a more thorough explication of those processes as well as a fuller view of the lives of the women and men who assisted the felters. Despite several readings of the relevant paragraphs and careful examination of the illustrations, I still do not understand how fur was transformed into felt. I also wonder whether male felters married the daughters of felters, what life expectancy was, just how dangerous the mercury solution was for workers, whether hatting was a prestigious craft, and so forth, all questions that Sonenscher does not attempt to answer. To combine the questions of political theory with those of social history, as Sonenscher has done in this volume, is unusual, and the book should interest scholars in both

fields. Both groups will probably find his use of sources and his coverage of the issues that interest them too brief, as I do, but he makes a persuasive case for rethinking the distinction between rights and customs and for examining the relationships among culture, political theory, and work, while he greatly expands our knowledge of hat making.

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ROBERT M. SCHWARTZ. *Policing the Poor in Eighteenth-Century France*. Chapel Hill: University of North Carolina Press. 1988. Pp. xix, 321. \$45.00.

Robert M. Schwartz has selected Lower Normandy (the *généralité* of Caen) as the focus of his study on the treatment of the poor from 1724 to 1789. What does the study of this region tell us that we do not already know from the research of Olwen H. Hufton, Jean-Pierre Gutton, and Cissie C. Fairchild? Schwartz's original contribution goes beyond another regional variation of these earlier works. Much like Steven L. Kaplan in his work on the grain trade, Schwartz concentrates on the evolution of national policy, or "policing," and its interplay with popular attitudes toward a growing underclass in French society. By concentrating on three phases of this policy (1724–32, 1764–67, and 1775–89), he is able to chart changes in governmental objectives, in the categories of poor confined to local hospitals and *dépôts de mendicité*, and in the attitudes of officials as well as in those of the rural and urban inhabitants of the *généralité*. His main conclusions are that the central government was much more effective in attaining its objectives than Hufton, Gutton, and Fairchild have maintained, largely because its objectives changed from attempting the elimination (Schwartz prefers "repression") of all begging to selectively incarcerating vagrants and the "criminal poor." His second conclusion is that this change of policy is related to a hardening of popular attitudes toward the poor that became fully apparent after Turgot attempted to release the inmates of the *dépôts* in 1775. Schwartz concludes his monograph with a telling episode, local inhabitants firing on escapees from Beaulieu *dépôt* outside of Caen in August 1789, a month after the inmates of the Bastille had been freed.

This sketch hardly does justice to the richness of this study, one that combines an exhaustive quantitative analysis of the registers of inmates of the General Hospital of Caen and of the *dépôt* of Beaulieu with case studies taken from police records of the *maréchaussée*. While the quantitative and tabular apparatus sometimes seems unneces-

sarily heavy for the conclusions obtained, the case studies—Namur, beggar and drifter, accused of leading a band of *attroupés* near Isigny; Jacques Grenier, *journalier ambulant*, floating from job to job all over the province, accused of stealing a silver watch in the local tavern; the classic servant girl denounced by the curé and notable parishioners for *libertinage*—all remind us of the vivid evocations of Hufton, Robert Darnton, and Richard Cobb. Schwartz's characters have human faces, whether they be conscientious ministers or petty officials, desperate beggars or fearful micro-proprietors.

On balance, the delicate integration of quantitative and qualitative social history is successful. My quibble is with Schwartz's occasional tendency to push the implications of his solid research too far: "In Lower Normandy and elsewhere in France the combination of repression and relief-giving enlarged the degree of control that could be exercised over an emerging proletariat made up of *journaliers*, petty artisans, and casual laborers" (p. 129). He also suggests that there were "ways in which the hospitals were related to the continuing development of capitalism" (p. 125). Is Schwartz suggesting that the state was fostering a "reserve army" by the palliative of public relief? These speculations, however, are not pursued. Given the tiny fraction of pauperism reached by those four thousand mounted police in a country of eight to ten million poor, one wonders how much impact the "policing of the poor" could have had on the labor force as a whole. Alexis de Tocqueville, not Karl Marx, seems most honored by this study. That is praise enough.

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GEORGE C. COMNINEL. *Rethinking the French Revolution: Marxism and the Revisionist Challenge*. New York: Verso. 1987. Pp. xiii, 225. \$34.95.

This book is an extended historiographical essay that runs against the grain of most recent writing about the French revolution. While historians on both sides of the Atlantic have become increasingly preoccupied with the political culture of Old Regime and revolutionary France, the debate over the Marxist interpretation has died down, largely for lack of new perspectives on the "social interpretation" once so loudly condemned by Alfred Cobban. In this essay, the Marxist interpretation is revived but with a twist. George C. Comninel is sympathetic to the aims of the leading Marxist war-horses—Albert Soboul, Claude Mazauric, and even Régine Robin, despite Robin's important revisions of the orthodoxy—but Comninel argues

that they, along with Marx himself, have missed the boat when it comes to the crucial concept of bourgeois revolution. Whereas "revisionist" (anti-Marxist) historians have long refused the concept of bourgeois revolution on the grounds that the historical evidence could not support it, the author of this book insists that it is not even consistent with the methodological principles of Marx's own mature studies and that in origins it is a suspiciously "liberal" conception.

Not that the author wishes therefore to dispense with Marxist categories; he aims rather to rehabilitate them through a careful rereading of Marx and Marxist historical accounts. This rereading will be useful to students of Marxism. Unfortunately, it is less clear that it will reinvigorate debates about the French revolution. It may well be that Marx's discovery of the alienation of labor as the basis of property was the critical development differentiating historical materialism from liberal materialist history (the point of a central chapter). We might accept that Marx's analysis of precapitalist class society was hopelessly inferior to his analysis of mature capitalist social relations because he followed the liberal historians too closely. Readers might even believe that "[Marx's] overall conception of history will be vindicated" if we follow the lead given by E. P. Thompson and Robert Brenner (p. 176). But will twenty-five pages in a conclusion finally devoted to the French revolution convince us?

Conviction is hardly likely to follow from an account that emphasizes "the central role of the state in the surplus extractive relations of the ruling class" (p. 200) but does not even mention Theda Skocpol. Or from an analysis of agricultural production that does little with recent, even avowedly Marxist, work such as Florence Gauthier's on Picardy. Most telling, however, is the complete absence of any reckoning with the growing influence of studies of political culture, which were renewed by François Furet and have been recently carried further by Keith Baker, Dale Van Kley, Mona Ozouf, and many others. Furet appears here only as a critic of the Marxist account, and, as a result, the emerging neo-Tocquevillian synthesis is ignored in favor of a rather more tortured, if intelligent, rehash of old issues.

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MICHAEL L. KENNEDY. *The Jacobin Clubs in the French Revolution: The Middle Years*. Princeton: Princeton University Press. 1988. Pp. xi, 440. \$45.00.

An author of any general history must choose one of two strategies: coverage, with the risk that

interpretation will be sacrificed to comprehensiveness; or analysis, with the risk that the history's value as a work of reference will be lost in ephemeral polemic. In the second volume of his projected trilogy on the Jacobin clubs, which covers the period from October 1791 to June 1793, Michael L. Kennedy has chosen coverage, presenting, as he says, "less a thesis than a narration" (p. ix). As in the first volume, the result is a truly impressive piece of archival research. He has uncovered information on more than seven hundred clubs in no less than sixty-six provincial repositories, not to mention demonstrating an unparalleled mastery of other primary and secondary materials. As a universal history, this study is not likely to be replaced.

Some of the author's conclusions are quite unexpected. One is the lack of influence of Robespierre and his friends who later became the Montagnards. Robespierre's newspaper was not widely read, his antiwar speeches were ignored, and his association with Marat and consequently with the September Massacres of 1792 brought his reputation to an all-time low. Only the Girondins' inept demand that Louis XVI's fate be settled by referendum marked the beginning of the Montagnards' return to favor in the clubs. Equally interesting is how rarely the provincial clubs were in the vanguard of national opinion. True, over 90 percent favored an offensive war in the spring of 1792, but, right up to the overthrow of the monarchy, few clubs were overtly republican. Over half of them came to demand the dethronement of Louis XVI quite late, and most wished to retain a monarchical constitution until the Convention declared the republic in September.

Kennedy is at his best when he analyzes in this precise fashion the clubs' reactions to national events. He ought to do it more often. His narrative strategy means that it is next to impossible to address the problem of Jacobin mobilization in this period. The traditional view is that the influx of new members from below was responsible for the clubs' increasing radicalism. It is equally likely that old-time club officers, increasingly worried about a deteriorating political situation, recruited a wider membership with their demagogic attacks on the rich. Kennedy has the material to assess these viewpoints but does not attempt to do so. One also wonders how reputations were won and lost. Robespierre appears in this volume to have been in eclipse, while in the earlier one he and Jérôme Pétion de Villeneuve steered the Jacobin club through the Varennes crisis and saved the network from the Feuillant schism. Again the elements for explaining this shift are here, but Kennedy does not bring them together.

In fact, narrative strategies often accept older

frameworks of interpretation inadvertently. Much of Albert Mathiez's view of national politics underlies this book, and much of that view is troubling: the habit of labeling clubs Girondin or Montagnard, the insistence on the importance of class struggle, the disinclination to take popular opposition to the revolution seriously, and so on. In short, Kennedy should inject more of himself into the final volume, which will surely be the most important recounting of the Jacobins' troubled history.

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THOMAS D. BECK and MARTHA W. BECK. *French Notables: Reflections of Industrialization and Regionalism*. (American University Studies; Series IX, History, number 24.) New York: Peter Lang. 1987. Pp. 254. \$39.30.

This book focuses on elites, regions, and economic growth, and their interrelationships, mainly under the July Monarchy. Besides electoral lists, sources include economic censuses of the period, and Thomas D. Beck and Martha W. Beck start with a good survey of economic trends. France's industrial growth, including landed and artisanal interests, was, they say, more evolutionary and mixed than England's, and there was no French counterpart to the English agricultural revolution.

For the "elite" paying a minimum of two hundred francs in taxes per annum, the land tax was most important, the House tax and Napoleon's Doors and Windows tax minor, and the *patente* (a licensing tax for business) increasingly significant. The Becks identify five major categories of the elite—landed, farming, business, professions, and government—and, within those, numerous subcategories and occupations, such as the important food processors. The authors drop the percentage of "landed" elite from Sherman Kent's 86 percent (1937) to 56 percent. For the business category, the northern departments had the highest percentage, artisan electors were concentrated heaviest in cities, and mountainous areas produced a disproportionate number of voting lawyers and notaries (notaries still to be studied, say the Becks).

The minimum annual income for a landed elector paying two hundred francs was sixteen hundred francs, with net worth about twenty times that amount. Even for the nobility, the distinguishing characteristic was great wealth. (A new *Robe and Sword* on this development would still be welcome.) A key theme on rural areas is that "farmer electors led the advance toward modernization" (p. 110), particularly in food process-

ing, and that the elite was being transformed from "idle landed" to people "engaged in the economy" (p. 111). The authors find that common lands everywhere were an index of backwardness and potatoes, hemp, and animals the big show in French agricultural development. They say we need more study of horses and food processing. The final chapter reiterates their themes and gives a common denominator for the elites: "The single force that held this diverse group together was opposition to those below them" (p. 151). Only after 1870 would the combined dominance of nobles, *propriétaires*, and the growing number of business and farmer elites end.

Pluses abound in this monograph. It is good to hear that Balzac was more correct on the elite than recently thought and good to have vignettes of top departmental taxpayers as well as suggestions to hatch dissertations, especially on north versus south. But there are minuses, too. The book is too complex and at times cheerless, and few will read every line, not to mention seventy-five pages of appendixes. Sherman Kent, who offered the Becks these lists, should also have left them his elegant clarity.

There are too many meaningless statements here: for example, "In general, the Landed were as a whole more prevalent in rural areas" (p. 52), or "France had moved far beyond its eighteenth century economy by the 1840s" (p. 136). Too many qualified assertions read like the following: "Professional men may not have welcomed modernization to the extent we have always assumed" (p. 69).

A bigger problem is the multiplicity of labels one can apply to the elite. The authors themselves inadvertently admit the difficulty, noting that "more land, larger houses, and bigger businesses tended to go together" (p. 78) or that "the nobles especially had a great tendency to avoid listing their Business connections" (p. 89).

The Becks seem too intent here on proving theses, or revising them, and Valéry's adage of finding before searching remains good, corrective advice. If in a new, more general book they emphasize evocation and avoid trying to prove what cannot be definitively proven, they should fulfill the promise of this one.

BARNETT SINGER
University of Waterloo

ROGER PRICE. *A Social History of Nineteenth-Century France*. New York: Holmes and Meier. 1987. Pp. xii, 403.

Roger Price is an economic historian by training. His new survey of nineteenth-century French so-

cial history develops the theme of his earlier works that France "modernized" in the nineteenth century as a consequence of market integration and the spread of railways; his favorite French phrase is "crise d'adaptation." Although Price's use of "modernization" may stick in the throats of many Anglo-American social historians, he employs it fairly innocuously as shorthand for specific economic processes rather than as a means to describe less tangible changes in mental outlook. Price agrees with David Pinkney that the essential dividing point in the history of modern France was the 1840s, not the French revolution; like Eugen Weber, he sees the era of the long depression of the 1870s and 1880s as the second major turning point, when the innovations of the 1840s bore fruit.

There is much to admire in Price's book. It is well organized and written in a straightforward manner. After an introductory section on economy and demography, Price goes on to discuss major social groups. He closes with chapters on institutions: religion and education with a coda on the state. Each of these subjects is discussed clearly and in depth. As a specialist in the transformation of rural France, Price is especially sensitive to regional differences. One feature that sets his volume apart from other surveys is his judicious use of unpublished French dissertations for local material.

Everyone who teaches and researches French social history would, of course, write a synthetic work in the field differently. Although no one would advocate a longer volume, I would have pushed for a somewhat more expeditious treatment of a host of topics. This approach would have opened up room for the more adventure-some project of bringing together new areas of research in social history, which receive little or no mention in Price's account, and the social history mainstream that he covers so well.

From his early study of the Second Republic through this book, Price has seen his work as a challenge to Marxist historiography on France. For the past two decades, however, many social historians in France and the United States have turned less to Marx for inspiration than to Michel Foucault. These historians have focused their attention on a variety of topics important not only in themselves but also for the ways in which they reveal the attempts of dominant groups to define and control the nature and norms of society, as well as efforts to resist these measures. Works in this vein cover a variety of "marginal" topics that receive little attention in Price's survey: Algeria, the police, crime, prisons, psychology, and, more generally, the anxieties, fears, and their purveyors that Theodore Zeldin probed in *France, 1848-*

1945 (1973 and 1977). Price makes good use of Louise Tilly and Joan Scott's *Women, Work and Family* (1978), except for mislabeling one of their tables on crude birth rates per thousand as "birth rate (percentages)" (p. 223). But greater attention could also have been devoted to questions of gender.

Tackling this more comprehensive project would present challenges because the methodologies and periodizations in the newer fields of social history differ from those in the more traditional regional social histories that provide the core of Price's sources. Yet such an effort could also open up new avenues of inquiry for social historians. Price's work is an excellent summation of past work for students; a different kind of book would be required to set an agenda for future research.

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ALBERT BOIME. *Hollow Icons: The Politics of Sculpture in Nineteenth-Century France*. Kent, Ohio: Kent State University Press. 1987. Pp. 163. Cloth \$18.00, paper \$11.00.

Albert Boime, an art historian, has briefly sketched out the politics of sculpture in nineteenth-century France. Given the medium of sculpture, sculptors needed state sponsorship to finance them and to provide the public spaces in which to display their works. Sculptors were thus more dependent on state patronage than painters and tended to be more conservative. Their dependence on the political and economic elites of the day made them support the status quo. Artists seemed perfectly capable of loyalty to the state regardless of changes in regime. Lifelong republicans such as David d'Angers, one of the most brilliant sculptors of the first half of the nineteenth century, did not hesitate to serve the three successive kings of the Restoration and the July Monarchy. David erected symbols that mobilized public opinion in favor of regimes antithetical to his values; he carved the Grand Condé in 1827 and then the pediment of the Pantheon in the 1830s.

Well documented in this book is the preoccupation of the French state with statuary as political statement. For those unacquainted with the subject of this work, it may come as a surprise to discover the extent to which rulers and their ministers busied themselves with public sculpture, concerned lest part of the symbol be misunderstood, demanding of artists changes that in nearly all cases the artists were but too willing to provide.

Governments spent considerable sums destroying statues and symbols of preceding regimes,

restoring those that had been victimized by foes, and building new ones glorifying their supporters. Given the large costs of public statuary, once the work of art had been begun, even if the political situation changed, governments would at times have the monument subtly modified to fit the new situation. Artists did not want to lose the opportunity to have their work erected, and governments did not want to see past expenditures spent in vain.

Boime's method of selection of these works of art is not always clear. According to what principles were sculptors and their works chosen for discussion? The column of Napoleon on the Place de Vendôme, the Arc de Triomphe, and the Pantheon are natural enough choices, but some of the other works seem to have been chosen because they were part of the corpus of a known sculptor rather than because they had any particular political impact. Furthermore, nearly without exception the works discussed are Parisian, which ignores the rich and important statuary in the provinces. The involvement of the central government is also overstressed; much public statuary resulted from the initiatives of local government and private bodies (although Boime brilliantly displays the impact of one such private group in the building of the Statue of Liberty).

Boime seems to have relied exclusively on published materials and to have finished writing the book prior to the appearance of the very useful series (by now four volumes) *Les lieux de la mémoire* (1985–87), edited by Pierre Nora. Some of the essays in that work would have helped Boime further elaborate several of his arguments. Boime's work is highly suggestive but by no means exhaustive. There are rich archival materials in both Paris and the provinces that would allow further development of the large and important themes that Boime has explored.

It is a measure of Boime's success that one would have liked to have read a bigger book that would have told us even more about the iconography of power in nineteenth-century France.

WILLIAM B. COHEN
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THOMAS J. ADRIANCE. *The Last Gaiter Button: A Study of the Mobilization and Concentration of the French Army in the War of 1870*. (Contributions in Military Studies, number 73.) New York: Greenwood Press. 1987. Pp. xv, 174. \$32.95.

"Am in Belfort," General A. E. Michel wired to the French war minister on July 21, 1870. "Cannot find brigade; Cannot find commanding general; What must I do; Do not know where my regiments

are." Michel's well-known *cri de coeur* sums up rather succinctly the perplexity and confusion that reigned in the French army in the summer of 1870. By contrast, Prussian war minister Albrecht von Roon remembered the first two weeks of the Franco-Prussian War, between the dispatch of mobilization orders and the first battles, as among the most relaxed and carefree of his public career, when the mobilization plans drawn up by Helmut von Moltke's staff momentarily lifted the burdens of office from his shoulders.

Thomas J. Adriance reminds us just how *terrible* the opening months of the "*année terrible*" actually were for the French army. Quite apart from the usual indecisiveness of the emperor, the intrigues of his wife, and the utter incompetence of Marshals Bazaine and MacMahon, the French army proved incapable of organizing and supplying its mobilization and concentration. The reasons lay with an intendance that was too small, lacked initiative, endured poor relations with the command, and had too few auxiliary services to support even a relatively small army. These problems made it difficult for the intendance to retrieve the initial mistake of the high command, which decided to dispatch the regular troops to the front, supply them, and then have the reserves join them later. As a consequence, the railroads were hurled into such confusion by the supply effort that few reservists ever reached their units. The French army marched into battle both undersupplied and undermanned.

This study offers, in effect, a brief history of the opening weeks of the Franco-Prussian War until the surrender at Sedan on September 2, 1870 that concentrates on the problems of mobilization and organization and on their impact on the campaign. It is balanced and well written. The author argues that the confusion of mobilization and supply intensified the natural caution of French commanders and limited their strategic options, even had they been inclined to do something more dramatic than simply retreat in confusion. Adriance's analysis is perhaps weakest in explaining why the French were unable to reform a system that they recognized as defective at least as early as 1866, and here the book would have benefited from some of the observations made by Richard Holmes in *The Road to Sedan* (1984). Adriance concludes that, whatever the failings of the French army in 1914, mobilization and supply were not among them and actually contributed to the French *redressment* on the Marne. Perhaps he should make that the subject of his next study.

DOUGLAS PORCH
The Citadel

FRANÇOISE THEBAUD. *Quand nos grand-mères donnaient la vie: La Maternité en France dans l'entre-deux-guerres.* (Collection Médecine et Société.) Lyon: Presses Universitaires de Lyon. 1986. Pp. 315. 110 fr.

Françoise Thébaud argues that "giving life" is much more than a mere biological act. It is a human activity whose frequency, methods, and location reflect the level of social development, economic growth and modernization, medical advancements, and the place of women in a particular culture. As such, it is a proper subject for study by historians. Indeed, during the last decade, the history of childbirth and maternity in France has become an important area of research for scholars on both sides of the Atlantic, and Thébaud's condensation of her excellent two-volume thesis is one of the prime examples of the new scholarship on the topic. This book delineates the interrelationships between demographic, institutional, and medical changes and women's childbirth experiences during the 1920s and 1930s in France. The medicalization of maternity during this interwar period had a profound effect on how women gave birth.

Part I contains a cursory examination of the conflict between the pronatalists and the neo-Malthusians over the role of women and asserts that the dominant ideology during the interwar period was that a woman, above all, was a mother. Pronatalist propagandists and politicians argued that to have children was a woman's duty, a source of her good health, and, most important, an act in the national interest. The struggle against the "social plagues" (tuberculosis, venereal disease, alcoholism, and urban slums) that contributed to the frightening fall in the birth rate and to the "massacre of infants" led to the emergence of social hygiene and the medicalization of French society.

In the second part of this monograph, Thébaud presents a carefully detailed articulation of the medicalization of maternity. Surveillance and education of the mother, both pre- and postnatal, to save the baby was the essence of this development. Doctors became the directors of hygienic childbirth and infant rearing, and they reorganized maternity hospitals to make childbirth more medical under their regulations and control. A thorough description of the Parisian public hospital Baudelocque and women's use of that hospital provides a case study of new medical processes and policies and of the most extensive medical and state intervention into maternity. Doctors did not stop their surveillance of mothers at the birth of their babies but developed well-baby clinics and programs of education in child rearing. Maternity

hospitals became effective centers of preventive medicine with maximum protection for mother and infant.

As a consequence of the medicalization of maternity, infant mortality declined, the clientele of the maternity hospitals increased and diversified as more women delivered in institutions rather than at home, and the profession of midwifery entered a crisis as obstetricians facilitated more deliveries. Thébaud's analysis is highly nuanced. Although childbirth and infancy were less perilous for mother and child, there was a loss of autonomy for mothers, especially working-class mothers, whom doctors viewed as minors who needed to be advised, supervised, directed, and controlled. Part 3 of the book is the most intriguing because Thébaud focuses on the women themselves and attempts to portray their attitudes and behavior. Using novels, journalistic accounts, and survey results, she shows that childbirth was a period of suffering, pain, isolation, and depersonalization for many mothers, although it was better to be well-to-do than poor. Even with the limitation of sources, having set herself the task of assessing maternity from the point of view of women, Thébaud could have accomplished this in a more complete, imaginative, and sophisticated manner.

The book provides a detailed example of the implementation of the "tutelary apparatus" that Jacques Donzelot analyzed in his *Policing of Families* (1979). Although Thébaud does not mention Donzelot's work, she clearly demonstrates the validity of his social control model for mothers in the 1920s and 1930s. The problems with this book are few: the paucity of footnotes and references might disturb readers who seek further information; in discussions of the consequences of medicalization of maternity, the conclusions frequently appear without sufficient evidence; and in several places Thébaud is repetitive. Although the title states that the book discusses maternity in France, it really concentrates on Paris with only a few paragraphs on other regions. These criticisms are minor, and the problems do not significantly mar an important analysis of the interrelationship between medicine and maternity during a twenty-year period.

RACHEL G. FUCHS
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ALAIN TOURAINE *et al.* *The Workers' Movement.* Translated by IAN PATTERSON. New York: Cambridge University Press and Editions de la Maison des Sciences de L'Homme, Paris. 1987. Pp. xx, 322. \$49.50.

Originally published in French in 1984, this very able translation results from a joint publishing agreement between the Fondation de la Maison des Sciences de l'Homme and the Cambridge University Press. Further volumes are to be expected, thus making many major works quickly available in English. Alain Touraine and his collaborators, sociologists by profession and preference, do make references to, and thoughtful, even challenging observations about, the history of the workers' movement. Their study has its base nearly exclusively in the French experience, although they attempt to relate their findings to other Western nations. In their conduct of lengthy discussions with workers between 1979 and 1983, they employed methods of "sociological intervention," which aim to avoid "epic-sounding descriptions" of workers' actions and to allow participants "a context in which they have the maximum possible freedom of analysis and argument" (pp. 50, 51). In this instance, the participants were iron and steel workers from Lorraine, railway workers from Perigueux, chemical workers from Lyons, unskilled workers from the Paris region, plus computer technicians and some managers. In the view of Touraine and his colleagues, these discussions constitute the raw materials of the study and are significantly less contaminated by values, ideologies, or current issues than materials normally used.

The purpose of the study, briefly put, is not to describe trade-union practice but to explore why and when union action "calls into question the mode of social management of industry" (p. xv). The analysis rests on the thesis that the workers' movement is a social agent, defined by "conflictual relations" and by positive reference to the cultural orientation of industrial society (p. xv). Thus, it would be erroneous to assume, as some historians have done, that the political posture of the workers' movement, for example, reformism or revolution, is necessarily definitive or that the alternatives are antithetical. Indeed, the political posture will vary, since the workers' movement is based on consciousness of a structural conflict over the resources of industrial society, which will be most acute when skilled and unskilled workers share a common experience. Technological changes and differentiation of skills and rewards, which remove the traditional category of worker, have resulted in the fragmentation and decline of the workers' movement in postindustrial society.

Touraine and his collaborators seek primarily to examine the present and look to the future. Although "social movements do not go on forever" (p. 290), they suggest that there is reason to think that trade-union activity may be spurred by new issues, but, with the passing of industrial society,

the workers' movement may too have passed. This study is an important contribution to the understanding of industrial societies and, as such, has rewards for historians. Engagement with their approach, but obviously without the benefit of the methods of sociological intervention, could lead to wider understanding of movements and actions, such as syndicalism, the strikes of 1936, or even the events of 1968, in the context of the industrial era.

NATHANAEL GREENE
Wesleyan University

NICOLAS SANCHEZ-ALBORNOZ, editor. *The Economic Modernization of Spain, 1830-1930*. Translated by KAREN POWERS and MANUEL SANUDO. New York: New York University Press. 1987. Pp. xv, 295. \$45.00.

This book is a collective effort consisting of fifteen contributions by fourteen authors. Despite the volume's common theme and the attention devoted to revising and editing, it remains difficult in a short review to be both comprehensive and constructively critical of such a work. The book has two main sections: the first is national in scope and includes seven essays on sectors of the economy (demography, agriculture, industrialization, transportation, finance, foreign trade, and the state); the second retraces the same ground by region, examining Catalonia, the Basque provinces, Asturias, Galicia, Castile, Andalusia, and Valencia. The authors include the best of Spain's economic historians: Rafael Anes, Albert Carreras, José Luis García Delgado, Pedro Fraile, Jaime García-Lombardero, Antonio Gómez-Mendoza, Jordi Maluquer, Pablo Martín Aceña, Jordi Nadal, Jordi Palafox, Vicente Pérez Moreda, Leandro Prados, Nicolás Sánchez-Albornoz, Pedro Tedde, and Gabriel Tortella. Explicitly or implicitly, most contributions are oriented to the problems of explaining the relative backwardness of Spain and its failure to modernize as did England, France, and Germany. This is not a new issue, but the book is extremely valuable because it brings home to historians who do not read Spanish a major development in Spanish historiography.

Older views of Spain's development emphasize persistent stagnation, especially in the agricultural interior, highlighted by an abortive flurry of development in the middle decades of the century. One variant of this idea suggests that a backward, "feudal" Castile held back a forward-looking, "capitalist" Catalonia. Development was aborted by the sale of major resources to foreign interests and by the dominance of foreign investors in modern sectors, investors who siphoned wealth

out of the country. The loss of America after 1800 crippled the beginnings of modernity in eighteenth-century Catalonia, while dominant agrarian interests preferred to preserve regional social control rather than to expand the larger economy. This situation led to tariff protection for agriculture and correspondingly high food prices. High food prices drove up labor costs, leaving industry uncompetitive abroad and confined to a narrow domestic market.

Although aspects of that scenario remain, this book gives us another view and points to a lively debate. Nineteenth-century Spain apparently experienced sustained economic expansion, making it different from many Third World countries today, although the rate of expansion and degree of structural change did not match those of England, France, and Germany. The export sector, although heavily dependent on agriculture, expanded much faster than the general economy and actually enjoyed favorable terms of trade through most of the nineteenth century. Agriculture (except possibly in Castile) emerges in these essays as frequently innovative and profitable, adapting rapidly to European markets. Spain's real problems were partly ecological: marginal agriculture and relatively high-cost transport (even with railroads) made market integration more difficult than it was elsewhere. Some contributors to this collection see a failure of nerve, not in agriculture but in industry. Industrialists opted for high tariffs and settled for the domestic market. They did not seek out competitive niches abroad, which would have increased output to more cost-effective levels and absorbed more labor. This choice meant that urbanization was slow and left agriculture oversupplied with labor. Hence, the agricultural sector made adaptations based on cheap labor rather than on labor-saving industrial inputs.

Certain villains figure in both scenarios. Each view contends that tariffs led to a self-contained, self-limiting economy, but the newer interpretation suggests that the manufacturers were the real culprits. Both scenarios comment on the rickety banking and financial system, which provided little credit to agriculture and may have transferred capital from the successful regions to the unproductive state. Finally, both versions suggest that industry was limited by weak demand. The question is whether inadequate demand was a result of the large, impoverished agricultural sector, the export of domestic wealth by foreign enterprises, or the failure of manufacturers to supplement domestic markets with foreign ones, a key factor in most successful cases of industrialization. This book shifts attention from the first two reasons and raises the possibility of the third.

The translation is uneven, and some essays do not read smoothly in English. I would hesitate to assign the whole book to a class, but several chapters are essential to any survey of Spain's history. My biggest reservation is that the book is too focused on explaining the differences between the Spanish economy and those of other countries. We need to go further and ask how the new perception of Spain's economy relates to the social and political process that erected tariffs, accepted an omnivorous national debt, and lived with an inadequate banking system. Even so, the volume is refreshing in its insights into a subfield where major revisions are underway. It presents the outlines of a new perception of Spain, one that shows Spain to have been an integral part of European modernization.

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ANGELES LEON EGIDO. *La concepción de la política exterior Española durante la II República (1931–1936)*. Madrid: Universidad Nacional de Educación a Distancia. 1987. Pp. 653. 3,200 ptas.

Angeles León Egido has made extensive use of Madrid's vast archival material as well as the major Madrid, Barcelona, and Valencia newspapers, party papers, magazines, and journals. Although it is true that local newspapers frequently contained little foreign news, one wonders whether papers in such smaller cities as Zaragoza, Seville, Bilbao, and La Coruña, among others, simply echoed the views of those in the three largest cities. León Egido has also examined many printed diaries and memoirs as well as numerous political tracts. Her use of foreign sources seems to be limited by language barriers. Although she has used sources in French, her English, American, Italian, and Russian citations are from Castilian editions.

León Egido writes that this work is a "revised version, considerably reduced in some aspects and considerably augmented in others," of her 1985 doctoral thesis. It both benefits and suffers from its origin.

The study is superbly organized. A brief chapter on Spain's role in international affairs is followed by chapters dealing with the views of liberals, conservatives, Carlists, fascists, intellectuals, socialists, Communists, and anarchists and by a final concluding chapter. Each chapter is clearly subdivided. The four-page table of contents gives a clear description of each section. One can quickly find the "basic objectives" of liberals, conserva-

tives, socialists, and Communists but not of Carlism, fascists, or anarchists.

The book's liability is its lack of an index—in Castilian, *índice* means both “index” and “table of contents,” but a table of contents is not a substitute for a reliable index. There is considerable material covering pre-1931 thought, but, unless one remembers where the statement of Donoso Cortes is, one has to search for it.

The book is well written, and the picture of massive paranoia (on the Left, fear of fascism, and on the Right, fear of communism) that distorted foreign policy attitudes clearly emerges. What is also apparent is that the defeat of 1898 did not end Spain's dream of empire.

The sections dealing with the fantasies of the conservatives and the fascists concerning Spain's mission in Africa and America are particularly fascinating. One is also intrigued by the blindness of some intellectuals who eschewed violence and espoused pacifism in the face of fascism. The author does not spare the Right, the Left, or the center in describing the mass self-deception that seemed to dominate Spanish thinking. Self-deception, unfortunately, seems to have been in fashion not only in Madrid but also in London, Paris, Rome, and Moscow. One is left with the impression that most of Spain's political leaders were too busy with ideas detailing the “decline of the West” or the “revolt of the masses” to bother checking reality.

Although the lack of an index does make life difficult for the scholar, the footnotes will be useful to those interested in culling printed material.

J. LEE SHNEIDMAN
Adelphi University

ANTONIO NOVOA. *Le temps des professeurs: Analyse socio-historique de la profession enseignante au Portugal (XVIII^e–XX^e siècle)*. In two volumes. Foreword by DANIEL HAMELINE. (Pedagogia, number 5.) Lisbon: Instituto Nacional de Investigação Científica. 1987. Pp. xxv, 514; 518–939. \$15.00.

This important study examines the teaching profession in Portugal from the sixteenth century to the twentieth century. The focus, however, is on two crucial periods, 1759–94 and 1901–33. The work embodies exhaustive research in Portuguese archival collections. A vast array of secondary studies was also consulted. The extensive bibliography in volume 2 is itself a valuable scholarly compilation.

António Novoa's wide-ranging research is complemented by a conceptual underpinning derived from the Annales school as well as from American

educational and sociological research. The result is a sophisticated, if eclectic, analytical framework that focuses on the role of the state in the evolution of primary education in Portugal. Novoa's work arguably stands as one of the most important studies of the history of Portuguese education yet published. It is at once a vital reference source and a touchstone for further research.

After detailing the various theoretical constructs that inform his study, Novoa provides a chapter on the genesis and development of the teaching profession in Europe, particularly in France. This chapter is especially useful in putting the concurrent Portuguese experience in context. Novoa then follows with a summary chapter on Portuguese education from the sixteenth century to the mid-eighteenth century. His discussion of the imposition of Jesuit control over education after 1540 is a useful synthesis that adroitly leads into the Pombaline reforms of the late eighteenth century.

Novoa devotes seven chapters to the Pombaline era. Here he examines the dual (and interrelated) themes of the secularization of education and the professionalization of the teaching force. Although reforms undertaken by the marquis of Pombal were of singular importance in the evolution of Portuguese education, they were by no means comprehensive. For example, instruction of females, first proposed for Portugal by Luís Verney in 1746, was all but ignored by Pombal. It was not until 1790, during the reign of Maria I, that the education of females and the admission of women into the teaching profession began in earnest.

In succeeding chapters Novoa describes the various nineteenth-century reforms that built on those of Pombal and Maria I. The Liberal revolution of 1820 and the Regeneration of 1851 were especially important in promoting reform. Thereafter, instruction of students—and their teachers—became more regularized and standardized. Novoa's discussion of the rise of normal school education is especially useful, albeit disappointingly brief. An account of the formation of teacher associations through 1897 rounds out volume 1.

Volume 2 details the progress of primary education in twentieth-century Portugal. The period 1910–26 is seen as a sort of golden age (*une période très riche*) in the history of Portuguese education. From 1900 to 1940 the teaching profession became increasingly feminized; the percentage of female primary instructors rose from one-third in 1900 to three-fourths in 1940.

Novoa shows that approximately one-half of the instructors traced their origins to rural regions, and of this group nearly 20 percent came from the rural petite bourgeoisie. Of the one-half of the

teachers from urban areas, approximately 23 percent had artisans or skilled workers as parents. Novoa also shows that 8.5 percent of primary instructors had parents who themselves were teachers. The author argues that this elevated percentage reflected a long tradition of passing positions from parents to children. Although salaries were minimal, entry into the teaching profession usually constituted a true *promotion socio-professionnelle*, especially in rural areas.

Novoa concludes with a discussion of the so-called New Education (*Educação Nova*), which derived from European pedagogical theory and gained increasing adherence in urban Portugal during the 1920s. The New Education took various forms, including instruction based on theory found in anarchist thought, which led to an approach that the author labels a sort of *utopie éducative*. The New Education was suppressed with establishment of the Salazar dictatorship, and one of its leading proponents, Alvaro V. Lemos, was imprisoned in 1934.

In an epilogue, Novoa briefly discusses the state of primary education in Portugal during the Salazar regime and the changes that came with the revolution of April 25, 1974. He shows, for example, that feminization of the profession continued through the Salazar years and that by 1983 nearly 92 percent of approximately 45,000 primary instructors were women.

Valuable as Novoa's study is, it is too much focused on the two great eras of reform: those of Pombal and the 1910 republic. As the author himself suggests, this dual emphasis detracts from his work's usefulness as a source on other periods, particularly the nineteenth century. The study would have been improved, for example, by greater attention to the rise of normal schools and the course of instruction offered in those institutions. Nonetheless, this book stands as a signal contribution to the history of Portuguese (and, for that matter, European) education.

CARL A. HANSON
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SVETLANA ALPERS. *Rembrandt's Enterprise: The Studio and the Market*. Chicago: University of Chicago Press. 1988. Pp. xvi, 60. \$29.95.

Svetlana Alpers's new book on Rembrandt promises to be as controversial as her previous monograph, *The Art of Describing* (1983). In some respects, this book offers a corrective to the rather narrow picture of Rembrandt presented there. After arguing for Rembrandt's "outsider" status in a Holland dominated by Baconian scientific knowledge and visual classification, Alpers here

turns about-face and contends that his art was a calculated response to the market economy of seventeenth-century Holland. This book will delight Alpers's admirers for her customary forcefulness of argumentation combined with numerous basic critical insights; at the same time, it will vex her critics, because it has the same fundamental desire interpretively to reinvent the wheel and draw constant distinctions between her insights and everybody else's (evident in her footnotes). This book provides a current index of the importance of criticism and deconstruction even within historical studies and should provoke discussions in a host of fall seminars on art history methods.

What emerges clearly from a careful reading of the book is its synthetic patchwork of the critical insights of other leading thinkers, whose works Alpers cites and then assimilates. One is more conscious of an intellectual genealogy here than a new historical contribution. The largest debt is owed to her colleague at Berkeley, Stephen Greenblatt, whose *Renaissance Self-Fashioning* (1980) continually informs and underlies this concept of Rembrandt creating an artistic persona for the conditions of the Dutch art market (pp. 5, 33, 59, 287). To Ernst Gombrich, Alpers's real *Doktorvater*, belongs the credit for wanting to pursue an etiology for the history of personal style, the analysis of "making" over "matching." For the concept of the "theatrical" in Rembrandt, Alpers has absorbed Michael Fried's *Absorption and Theatricality* (1980). In the final chapter, she cites Georg Simmel's *Philosophy of Money* (1900) as an analogue to Rembrandt's own appreciation of art as capital but not Simmel's own essay on Rembrandt (1916), which is quickly discounted (p. 153, n. 53).

Her debts to Rembrandt scholars, particularly Peter Schatborn on the drawings and Ben Broos on the documents, are enormous and acknowledged; however, Gary Schwartz's controversial interpretations in *Rembrandt: His Life, His Paintings* (1985), which often approximate (and often seem to have provoked) Alpers's own, are usually summarily dismissed without comment. Schwartz has already analyzed the failure of Rembrandt to work with court patrons and other patricians and boldly hypothesized about Rembrandt and theater and Rembrandt and the art market. Alpers's readers should also examine Schwartz's longer study.

There is more "Rembrandt" than "enterprise" here. Chiefly, Rembrandt's individualism is explained as the result of his desire for personal freedom from aristocratic patrons and as a market strategy (with the insight that Rembrandt began to use art as a form of promissory note) that substitutes personal "authority" for "authenticity." This is the ultimate in "Renaissance self-fashioning," defining Rembrandt in terms of "freedom, art,

and money" (Alpers here quotes an eighteenth-century critic, Jean Baptist Descamps).

There are numerous insights in this text, such as the (anachronistically expressionist) analysis of Rembrandt's brushwork (but, curiously, not his signature). Yet his brushwork, his theatricality, and his individualism have long been the subjects of Rembrandt scholars, and Alpers's analysis remains (like her previous consideration of empiricism) largely revisionist. This book does have topicality in its consideration of the reasons for the problems of authenticity raised by the team of Dutch scholars reattributing so many "Rembrandts" to followers or imitators, possibly within the artist's own studio.

Yet Rembrandt's enterprise still is scarcely situated within the larger framework of Rembrandt's Amsterdam; the emphasis here is on the artist's "mastery" within his own studio (pp. 58–87). Thus, Rembrandt's emulative relation to Rubens (his role model in the 1630s, when so many variants and copies came out of the Rembrandt studio) is discussed chiefly to draw distinctions. His "indenture" to debt in his final decade is perversely assessed in positive terms, as a "negotiation" rather than as an "obligation" (pp. 96–97). Most historians, therefore, will be better served for an analysis of Dutch values and the pervasiveness of the market economy by turning to Simon Schama's recent, rich monograph, *The Embarrassment of Riches* (1987).

LARRY SILVER
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EVA ÖSTERBERG and DAG LINDSTRÖM. *Crime and Social Control in Medieval and Early Modern Swedish Towns*. (Studia Historica Upsaliensia, number 152.) Uppsala, Sweden: Uppsala, distributed by Almqvist and Wiksell International, Stockholm. 1988. Pp. 169. 135 KR.

In the last twenty-five years, the history of crime has become a new subdiscipline with its own methodology and theoretical framework. The earlier anecdotal use of judicial sources has given way to a systematic analysis of court records, employing sophisticated quantitative and qualitative techniques. Focused originally on conditions in France and England during the early modern age, interest has steadily spread chronologically and geographically, resulting in an international debate on the causes of, and changes in, Western crime from medieval to modern times. This broad scope has invited comparisons and explanations and has credited perceived changes to factors such as the growth of the state or capitalism.

Language barriers often prevent scholars from

including Scandinavian conditions in their conclusions. This is regrettable because the more archaic quality of Nordic society preserved a profile lost elsewhere, thus supplying an important first step in the history of crime. Furthermore, although the Scandinavian medieval and early modern sources are fewer than those from other European countries, the more uniform judicial picture within the Nordic countries eliminates one of the many obstacles to drawing conclusions.

Thanks to the work of Eva Österberg and Dag Lindström, Swedish criminality in the medieval and early modern period can now be included in the international debate. In separate chapters, Österberg introduces the international scholarship on crime and provides the non-Swedish reader with a short survey of Swedish politics and society, and Lindström treats the laws, courts, and legal procedure of the period.

Based on court records and fine-payment rolls, the authors provide the first systematic analysis of Swedish crime between 1475 and 1625. Since rural Sweden has not preserved sources sufficient to permit international comparisons, Österberg chose Arboga and Vadstena as examples of provincial towns, whereas Lindström focuses on Stockholm. Both authors deal ably with the dark figure of unrecorded crime. With relatively abundant sources, Stockholm offers a detailed picture of crime that includes violence, defamation, theft, economic wrongdoings, disobedience, and sexual aggression.

Neither the provincial towns nor Stockholm lends support to the controversial thesis that sees a transition *de la violence au vol* in the early modern period. While other scholars have pointed out that theft was already fairly common in the late medieval period, the Swedish evidence demonstrates that violence remained the most frequent misdeed until the beginning of the seventeenth century, in spite of an increased crime rate. Violent offenses, however, consisted mainly of petty assaults committed by ordinary citizens, who brought even minor cuffs and bruises to court. This suggests that people were still quick to use physical force but unwilling to accept it from others. The low rate of theft is explained by the high percentage of Swedish freeholders (more than 50 percent) and the relative lack of urbanization.

Both authors treat the role played by the courts. They conclude that, although the relatively few serious offenders were punished as harshly as the law permitted (death or expulsion), in the majority of the cases courts sought the criminals' reintegration into society. This goal was achieved by consistently applying the law more leniently than prescribed by the codes.

The book ends with a summary on cruelty and

care within the family, a subject that Österberg has treated previously. Only a certain repetitiveness, especially in Lindström's chapters, mars this important and original contribution to the history of criminality.

JENNY JOCHENS
Towson State University

MAUNO JOKIPII. *Jatkosodan synty: Tutkimuksia Saksan ja Suomen sotilaallisesta yhteistyöstä 1940–41* [The Continuation War: An Investigation of German-Finnish Military Collaboration]. Helsinki: Otava. 1987. Pp. 748.

Mauno Jokipii's massive study is an exploration of a much-studied matter, the involvement of democratic Finland with Nazi Germany in war against the Soviet Union. A lively historical debate, which has generated a sizable literature, has been waged on this issue for decades now. In addition to Finnish historians, American, British, German, Russian, and Swedish scholars have participated in it to a significant extent. Jokipii's principal contribution to this scholarly enterprise consists of an examination of the nature and evolution of the Finno-German military cooperation that preceded the German invasion of the Soviet Union in June 1941 and the consequent renewal of war between Finland and the Soviet Union.

The question that Jokipii set himself, and proceeds to answer, concerns the extent to which Finland actually influenced its own fate. As is not unusual for small countries in an international crisis, the options open for Finland were circumscribed by forces and circumstances beyond its control. Jokipii seeks to find the answers to his question in concrete physical actions, in deeds rather than in words. From his research he concludes that Finland influenced its destiny more actively than some previous explorers of the topic have indicated.

Finnish military cooperation with Germany was the logical result of the Soviet invasion of Finland in 1939, the subsequent Finno-Soviet Winter War of 1939–40, and continuing Soviet pressure. Under this pressure Finland initially sought support in three principal directions: a neutral Scandinavian coalition or close collaboration with Sweden, aid from the Western powers, and, alternatively, backing from Germany. Soviet and German opposition doomed the Scandinavian option. German military successes in Scandinavia and Western Europe left Germany as the only viable, although not preferred, alternative for support against Soviet pressure.

Soviet incorporation of the southern neighbors of Finland—the Baltic republics of Estonia, Latvia,

and Lithuania in July–August 1940—intensified Finnish fears and suspicions of the Soviet Union. The fate of the Baltic republics and Soviet Foreign Minister V. M. Molotov's demand made to Adolf Hitler in Berlin in November 1940 for a free hand to liquidate Finland, on the basis of the Nazi-Soviet Pact of August 1939, spurred the Finns on to cooperation with Germany. Support for cooperation rested on the Finns' concern with a Soviet threat as well as a broad and deeply felt popular desire to gain the return of territories and cities lost in the Winter War. The refugee problems that had resulted from those losses played an important role in intensifying public opinions. Thus, the Finns also came to regard the renewal of war with the Soviet Union as a continuation of the Winter War that could correct its injustices.

The origins of this "Continuation War" have long been, besides a topic of scholarly interest, a sensitive political issue connected with the postwar Finno-Soviet relationship and a factor in postwar internal political infighting in Finland as well. In part as a result of these complications, many Finns preferred not to publicize fully the extent of Finland's active military preparations for a possible renewal of war with the Soviet Union. Some viewed certain aspects of such preparations as an admission of guilt or as acts against the national interest rather than as preparations for both the worst possible eventuality and the possibility of regaining at least some losses from the Winter War, thus healing an open wound and frustration in the national consciousness.

Jokipii has been able to build on extensive previous research and to synthesize it. He has made use of and complemented previous contributions to the topic by H. Peter Krosby, C. Leonard Lundin, Ohto Manninen, Tuomo Polvinen, Hannu Soikkanen, and Anthony F. Upton, among others. With his study, Jokipii does not change the broad existing scholarly picture on the subject, but he does enrich it with new nuances. His most original contribution of new knowledge rests on his careful and detailed examination of the concrete functioning and the facts of the Finno-German military cooperation. Both as an informative analysis and as a dispassionate synthesis, this book is a welcome addition to Finnish historical literature.

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LEO GOLDBERGER, editor. *The Rescue of the Danish Jews: Moral Courage under Stress*. New York: New York University Press. 1987. Pp. xxvii, 222. Cloth \$40.00, paper \$20.00.

The Germans attempted to kill the Jews of Europe in countries that Germany occupied or in those that were under German influence. Their success varied greatly—90 percent of Poland's Jews were annihilated, 20 percent of Italy's. None of Denmark's Jews were killed (although fifty-one died of natural causes in the Theresienstadt camp). The case of Denmark is unique. The population successfully opposed the German attempt to deport the Jews by mounting a rescue operation that permitted almost all Jews to escape to Sweden.

Following the invasion (which the Danes did not resist), Germany permitted the Danish government and parliament to continue to function. Until the summer of 1943 this arrangement functioned well. The Danes were largely able to hold on to their civil liberties (although in violation of the Danish constitution the Communist party was outlawed and Communists sent to a Danish prison camp). The Germans made no effort to ship Jews out of Denmark. These arrangements broke down in August 1943, when, amidst an increase of resistance activities, the Danish government resigned in the face of unacceptable German demands. The Germans declared a state of emergency and took power.

Georg Duckwitz, a German shipping expert who worked for the German embassy in Copenhagen, informed Hans Hedthof, a leading Social Democrat, of the impending plan to deport the Jews. The Jewish community was warned. Danish citizens in a unique show of solidarity at first hid Jews and then shipped them across the sound to Sweden, which was willing to accept them. (A Dutch strike to protest German actions was broken by the Germans.)

This volume grew out of a 1983 conference commemorating the fortieth anniversary of the rescue operation. The book contains three sections: a historical analysis, personal narratives of individuals, and essays dealing with the theme of "moral courage under stress."

The central essay by Jørgen Hæstrup, a leading Danish historian of the war years and the resistance movement, reviews Danish-German relations during the occupation and the events leading to the German attempt to deport the Jews and the successful attempts by the Danes to prevent this. The central section of the work presents personal narratives of Jews who escaped and of members of the resistance movement. Those narratives provide a personalized context for the events.

The final section attempts to explain the unique actions of the Danes—so sharply in contrast to those of the peoples in many other countries of Europe who were willing to cooperate with the Germans or were unwilling, at least, to take any risks to save their Jewish fellow citizens. Beyond

the obvious answers—the absence in Denmark of the kind of terror that prevailed in other occupied countries because the Germans wished to make Denmark a model occupied state, the almost complete absence of Danish anti-Semitism, the proximity of Sweden and its willingness to accept Jewish refugees—several contributors to this book emphasize that the Danes regarded the Jews as fellow citizens, as part of their community. Samuel Abrahamsen speaks of the Danish *livskunst*—the art of living in a society that accepted religious differences, cooperation, and self-reliance (p. 8). The use by Otto F. Kernberg of Wilfred Bion's small-group theory is suggestive, though in need of further development.

This work fulfills its aim of recounting and commemorating an exceptional event that deserves remembrance and paying tribute to the collective and heroic efforts of the Danish people in rescuing, at substantial risk, their Jewish fellow citizens.

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HANS J. HILLERBRAND, editor. *Radical Tendencies in the Reformation: Divergent Perspectives*. (Sixteenth Century Essays and Studies, number 9.) Kirksville, Mo.: Sixteenth Century Journal. 1988. Pp. 140. \$25.00.

Ever since the sixteenth century, the radicals of the German Reformation have been a thorn in the side of scholars. Broadly branded as *Schwärmer* by Martin Luther, this disparate group of Anabaptists, Spiritualists, apocalyptic warriors, and (since George Williams's *Radical Reformation* included them) Unitarians seems to share nothing save rejection of and by both Catholics and mainstream Protestants (that is, Lutherans and the Reformed). The involvement of certain radicals in both the Peasants' War and the Anabaptist Kingdom of Münster has also made them a point of interest for those who study the political and social upheavals of the period. It is to these issues that the present volume is primarily devoted. It contains seven articles that were originally presented at the Sixteenth Century Studies Conference in St. Louis in October 1986. The participants, three North Americans and four East Germans, presented interpretations of movements and individuals that are generally considered radical. Although both the sessions at St. Louis and the resulting book also serve to make clear the changes that Western treatments of the radicals have undergone, in large measure they were designed to introduce Western scholars to recent East German historiography on the German Reformation. And it is on

East German historiography that I will concentrate.

What becomes quite clear is that, despite the increased interest and sophistication of Western scholars in the study of social and economic factors in the Reformation, presuppositions and procedure still separate East and West. The title *Radical Tendencies* (at the conference "Radical Currents") allowed the participants to range more widely than "Radical Reformation" might have done. Nonetheless, tacitly or explicitly most of the contributors agree with Hans Hillerbrand in defining "as radical reformers only those who undertook to alter the existing societal order on the basis of religion" (p. 29). This definition effectively excludes a wide range of individuals and groups normally considered radical but who possessed no social agenda. Sigrid Looss's paper provides convincing evidence that Bodenstein von Karlstadt should not be included for this reason. Also as a result, Siegfried Hoyer neglects at least one prominent lay preacher of the early years—Kaspar Schwenckfeld. One might well wonder if the individualistic rationalism of a Sebastian Franck or the peaceable sectarianism of the Mennonites were not inherently and in the long run more radical than the revolutionaries who continued to cherish a vision of *Christentum* that was essentially medieval. This concentration on the short-term and spectacular is further exacerbated for the East German scholars by their apparent inability to come to grips with religion per se, a failing shared by many in the West. The religious impulse is reduced to theology (Adolf Laube, p. 10) or viewed as an ideology aimed primarily at remaking this world (note the contrast of Günter Vogler and James Stayer in this regard). The academic study of premodern religion has revealed it to be a much more extensive, autonomous, and powerful phenomenon than most post-Enlightenment thinkers have been willing to admit.

But the East German contribution is to be sought elsewhere—in their effort to impose a conceptual framework that will discover the larger significance behind seemingly haphazard events. The venerable *Frühbürgerliche Revolution* has been further elaborated by the formulation of a *Bürgerliche-Radikale Reformation* and a *Volksreformatorische Bewegung* as competitors to the original Lutheran impulse. The effort to see the "forest" and not just the "trees" is to be applauded, even if in this case it does not fully succeed. For one thing, the framework was not consistently applied by Looss, Hoyer, or Vogler. More important, James Stayer's presentation of the findings of recent research on Münster, by making clear how much in the Anabaptist Kingdom and its ideology was the product of chance, short-term crisis man-

agement, and unforeseen consequences, undermines the theoretical preconception on which the East German model is based. One can only conclude that, despite the efforts of both East and West, the radicals remain for the time being the same prickly, obstinate, and undomesticated lot that haunted Luther and the sixteenth century.

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KATARINA SIEH-BURENS. *Oligarchie, Konfession und Politik im 16. Jahrhundert: Zur sozialen Verflechtung der Augsburger Bürgermeister und Stadtpfleger 1518–1618*. (Schriften der Philosophischen Fakultäten der Universität Augsburg; Historisch-sozialwissenschaftliche Reihe, number 29.) Munich: Ernst Vögel. 1986. Pp. 358.

Katarina Sieh-Burens's analysis rests on one premise: political behavior in Augsburg was predicated on four socioeconomic networks. Focusing on the seventy-three highest officeholders between 1518 and 1618 (the *Stadtpfleger* and *Bürgermeister*), the author reconstructs their social networks (*sozialer Netz*) by combing through family chronicles, genealogies, notarial records, business contracts, and topographical data. She then analyzes four confessional conflicts, using network affiliation to explain the behavior of the political elites.

Part 1 gives a succinct introduction to the socioeconomic and political structures of Augsburg, home to capitalist banking houses, a strong guild community, and a large proletariat. The reconstruction of networks, part 2, is the real substance of the study. Four categories of networking (*Verflechtung*) are presented: household and extended family ties, legal connections, business relations, and neighborhood. The author identifies four networks among the ruling elite: the Welser network, numerically predominant, diffused, open, and focused on the venerable patrician banking house; the Fugger network, a smaller, more cohesive and exclusive group, comprising the Fuggers, their business associates, political allies, and clients; the Herbtrot network, a small but politically vociferous group of rich merchant newcomers; and the Seitz network, a small and temporary faction in city government made up of middling guild members.

Part 3 analyzes four confessional conflicts from the perspective of network politics. During the years 1533 to 1537, cooperation between the Welser, Herbtrot, and Seitz networks overcame the opposition of the Catholic Fugger group and adopted the Reformation, although individual Protestant magistrates supported different theo-

logical positions. In the years leading up to the Schmalkaldic war (1546–47), the Herbröt network, the war party, dominated and radicalized politics, steering Augsburg away from the imperial cause to the Protestant camp. The victory of Charles V ushered in a new civic regime. With the abolition of guilds and the restoration of Catholicism, the Fugger network entered Augsburg politics as clients of the Habsburgs. The abortive guild revolt in 1552 ended the political role of the Herbröt network. After 1552, the Welser and Fugger networks dominated Augsburg. The extensive Welser network, with Protestant and Catholic members, exerted a moderating influence on the more openly Counter Reformation Fugger group and succeeded in keeping religious peace in the city, as demonstrated by the role key Welser magistrates played in the controversy surrounding the introduction of the Gregorian calendar in 1583.

Behind the clarity of the network model lies a painstaking effort that assembled highly disparate data from widely different sources. Conceptually more comprehensive and abstract, network subsumes family, business, and patronage. The book is particularly good at describing the accumulation of power by the Fuggers through business connections, patronage, and marriage strategies. I find the analyses of the Herbröt and Welser networks equally convincing. More questionable, however, is the designation of the artisanal Seitz group as a network. Guild politics functioned very differently from the way patrician families or ambitious newcomers constructed power networks. The Seitz group's support for Zwinglianism reflected guild corporatism directed at precisely the network politics so ably analyzed by the author. For its lucid analysis, strong methodological approach, and new information, this study is an important addition to the field of Reformation studies.

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DEBORAH HERTZ. *Jewish High Society in Old Regime Berlin*. New Haven: Yale University Press. 1988. Pp. xiv, 299. \$30.00.

In late eighteenth-century Berlin, literary salons helped expand the awareness and appreciation of works of contemporary authors among the middle and upper classes. The social energies of these informal gatherings of men and women, however, had on the whole more staying power than did their intellectual drive. The salons added to the sophistication of urban society; they might also

have been avenues of upward social mobility for some of the young officials and professional men who attended them and for a few of the hostesses as well. Among these were daughters of wealthy Jewish families, and the salons they inspired and maintained by means of their intelligence, their personalities, and their determination to break out of the double isolation from which they suffered as Jews and women helped for a time to advance the processes of Jewish assimilation.

Deborah Hertz raises two central questions about the salons and their place in society: Who attended them? What motivated people's attendance? From biographical dictionaries, memoirs, and other secondary works she has identified 417 male and female "intellectuals" who lived in Berlin between 1780 and 1806 and has found that slightly less than one-fourth of this group attended at least one salon during this period. By studying their "social origins, occupations, friendships, love affairs, and intellectual works" (p. 20), she seeks to explain why some were drawn to the salons and others were not. A list of the names of these 417 and a reference in the literature for each would have been a useful appendix. The author has read widely, but a further search might generate additional names. Those she has identified give us some specific information, lacking until now, on the makeup and dynamic of the Jewish salons. The mix of reasons that brought people to a salon is, however, not very surprising: intellectual interest, business and professional contacts expanding into leisure hours, social ambition, even romantic and sexual fascination. Once having identified those, we soon reach a point where individual motivations escape the social historian's grasp. The statistical analyses are complemented by thoughtful discussions of aspects of the condition of Jews in Prussia at the time, for instance, a sensitive reconstruction of the prospects in life facing well-to-do Jewish girls. Hertz's objectivity should also be noted. She makes it plain, for example, that by no means all women at the center of salon society held progressive views on intellectual or social matters.

The book's qualities make it the more regrettable that the text shows signs of carelessness, not all of which can be attributed to the very haphazard proofreading. Jewish men make up 3 percent of Berlin intellectuals on page 57 but 4 percent on the chart two pages later. The table "Jewish Salon Women" (pp. 192–93) lists Marianne Solomon as both "single for life" and "divorced/widowed." On page 232, people between twenty and twenty-nine constitute "only a fifth" of Jewish converts between 1770 and 1799; on the following page they appear as nearly one-fourth. The accounts of Prussian history, society, and culture that frame

the analyses of the Jewish community are pitted with mistakes, some trivial, others more far-reaching, for instance, the surprising assertion that Prussia acquired Saxony as the result of the Seven Years' War (p. 26). It was not the case that before 1806 Prussian officers "could marry only upon reaching a high rank" (p. 219). One really cannot say that Prussia "had withdrawn from the anti-Napoleonic coalition" in 1795 (p. 252) nor that after 1811 "it would take two more years . . . before Prussia eventually joined with Austria in the war against France" (p. 273). "Reichsfreiherr von [*sic*] Stein" was dismissed as Prussian minister neither in 1806 nor for the second time in 1809, and Hardenberg was a prince neither in 1807 nor in 1809 (pp. 266, 268). Heinrich von Kleist was assuredly not one of the "four central figures" of the "Christlichdeutsch [*sic*] Tischgesellschaft" (pp. 271–72); indeed, some scholars doubt that he was ever a member at all. And so on.

A last citation may suggest how such inaccuracies encroach on the book's overall argument. One reason for the decline of the Berlin salons, the author believes, was that "the reforms of 1806 [?]—1814" created a "smaller, wealthier nobility," which was less "in need of loans and diversion, and . . . lived by a more exclusive social code." Consequently, the bourgeoisie now "had limited contact with a more elitist nobility" (p. 283). This is an odd view of Berlin society in the nineteenth century, which on the contrary was marked by interlocking circles of nobles and bourgeois, existing side by side with court society. It further ignores the large number of new nobles created throughout the period and the many noble-bourgeois and Jewish-gentile marriages. Numerous examples of both may be found, for instance, in the proliferating family tree of the Mendelssohns. And was it really a case of the salon declining, or were social institutions becoming more diverse? An example of the continuing vitality of the salon, particularly relevant to the concerns of this study, is the one begun in the 1830s, hosted by Franz Kugler's partly Jewish wife, Clara, which attracted poets, scholars (among them the young Jacob Burckhardt), officers, and officials.

If the book is uneven, however, it is also interesting. Some of the author's statistical analyses and her discussions of certain aspects of Jewish life in the last years of the old monarchy are welcome contributions to the study of women in European society and to the frequently linked history of the salon and of Jewish assimilation in Berlin.

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KARL-JOSEPH HUMMEL. *München in der Revolution von 1848/49*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 30.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 574. DM 162.

Munich in 1848 was not Berlin nor, it seems, a dozen other German cities. That is the thesis of Karl-Joseph Hummel's study, whose proper title might have been "Munich outside the Revolution of 1848–49." Hummel is intent on demonstrating that no revolution from below occurred in Munich, on explaining why this was so, and on providing instead that the capital of Bavaria witnessed reform from above. Otto von Bismarck was apparently not the first white revolutionary.

Hummel's detailed treatment of Bavaria's capital from 1847 to 1850 is divided into two almost self-contained halves. The first gives us a description of political developments, the second an extensive analysis of Munich's social and economic structure. The author's presentation is buttressed by 168 tables.

According to Hummel, no significant antimonarchical sentiment existed among Munich's population. Only Ludwig I's amorous blunders brought the king, not the monarchy, to a crisis. But a group of "liberal-conservative" officials, who favored a constitutional monarchy, took charge and steered the government through the crisis. They engineered Ludwig's abdication, which proved to be the turning point in 1848, acting not out of *Angst* but out of their own view of Bavaria's interests. Apart from Ludwig's removal, Hummel stresses, there was a continuity of personnel at the level of both the central government and the city administration. Even when voting rights were more democratic, as in the elections to the Frankfurt National Assembly, Munich's electorate returned strong moderate to conservative majorities.

Having detected little evidence of revolutionary intent but instead a desire to maintain the status quo, Hummel finds his explanation in an analysis of Munich's social and economic structure. He argues that developments from 1830 to 1848 did not create revolutionary tinder. Demographic changes occurred at a slow pace, and population growth was less than the German average. Overcrowding in the crafts, which other historians have stressed as an important factor, was absent in Munich, which profited from its political and cultural importance. Thus, Hummel detects no tension between journeymen and masters, both of whom were committed to the maintenance of the traditional guild system. Neither was there a significant proletariat, and workers were supposedly well treated. He even finds a decline in the num-

ber of dependent laborers and poor people, while the percentage of professional and upper-class elements grew. This rosy picture of Munich's population is bolstered by figures on food consumption that show Munich comparing favorably with Paris. Even statistics on banks deposits and criminality in 1848 confirm a basically stable society. Hummel concludes that there was no revolutionary group strong enough to begin a revolution, further radicalize it, or win it.

It is difficult to take issue with a study based on 168 tables and extensive archival investigations. The author's relentless hammering away at his thesis, however, raises questions about whether he could have approached his topic with the historian's responsibility to understand. He intends to show the importance and continuity of Bavarian history and, it seems to me, the connection between 1848 and 1948. His study reeks of self-satisfaction about Bavaria's ability to show the way to Germany-Prussia. Although he repeatedly mentions reforms from above, there seems relatively little that changed as a result of 1848. Rather than reform, it is the status quo that is the true outcome of Bavaria's development. Bismarck's reputation seems safe enough. Moreover, that conservative German governments instituted reforms in the wake of the 1848 upheaval is hardly news since Theodore Hamerow's work thirty years ago. Hummel spends much more time examining the conservative reformers than he does the democratic clubs. He goes to great lengths to prove what a good employer Maffei was but devotes little attention to understanding the lower-class elements. He dismisses examples of urban violence as the work of small unrepresentative groups or examples of poor coordination by the security forces. His conclusions may be correct, but the mode of presentation is likely to arouse doubts in all but the already committed.

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colonialism was of but marginal importance to the Wilhelmine Reich and that its main historical legacy is the existence of several African states whose boundaries it carved out.

Why then this volume? The answer, to put it mildly, is elusive. All of the authors—American (Lewis H. Gann, Richard V. Pierard, Woodruff D. Smith, Donna J. E. Maier, Arthur J. Knoll), East and West German (Helmut Stoecker, Peter Sebal, Winfried Baumgart), and Australian (Peter Hemenstall)—are well known to specialists for high-quality work on aspects of German colonial history. All of their contributions to this volume are technically competent, well written, and grounded in a wealth of secondary studies. Most of the essays summarize earlier work by the same authors; in the case of Pierard's article on the German Colonial Society, a 1964 dissertation.

The essays are highly diffuse in scope, approach, and subject matter, ranging from a discussion of slave labor and wage labor in German Togo (Maier) to a well-worn account of the German socialist opposition to colonialism (Stoecker and Sebal) and a general look at German imperialism in historical perspective (Baumgart). There is no unifying theme and little sign of new approaches or ideas, with the exception, perhaps, of Smith's piece on anthropology and German colonialism (which essentially concludes that the ties between the development of academic anthropology and Wilhelmine colonial practice were quite tenuous).

Specialists will find very little that is new in this volume. For the general reader the essays are too narrowly focused and too diffuse in their subjects to provide a survey of the history of Germany's colonial empire. Withal, this publication raises the question whether there are fields that have been plowed so thoroughly and so often that the soil is becoming sterile.

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ARTHUR J. KNOLL and LEWIS H. GANN, editors. *Germans in the Tropics: Essays in German Colonial History*. (Contributions in Comparative Colonial Studies, number 24.) New York: Greenwood. 1987. Pp. xiv, 178. \$39.95.

In his introductory essay to this volume, "Marginal Colonialism: The German Case," Lewis H. Gann makes the unchallengeable point that, considering the brief duration of Germany's career as a colonial power, academics and others have spilled more ink per square mile over the former German colonial territory than they have over any other empire in the world. He concludes that German

MARTIN H. GEYER. *Die Reichsknappschaft: Versicherungsreformen und Sozialpolitik im Bergbau 1900–1945*. Munich: C. H. Beck. 1987. Pp. 520. DM 38.

In the 1980s the centennial of Bismarckian social insurance legislation has combined with speculation about the future of the welfare state to stimulate renewed interest in the history of German social policy. As Martin H. Geyer notes in the introduction to his book, much of this interest has centered on origins and initial implementation. Geyer's own emphasis, in contrast, is on the response of long-established institutions to the trau-

matic challenges of the first half of the twentieth century with particular attention to the era of the Weimar Republic. Geyer devotes more than two hundred pages to the years between 1918 and 1933. Developments under the Third Reich are covered in fewer than half that number, those of the empire in fewer than a quarter.

The social insurance legislation of the 1880s aimed not at uniformity but at incorporating already existing providers even as it created new ones. Noteworthy among established providers were the miners' *Knappschaften*. With origins in the Middle Ages, these funds initially were largely untouched by the new national initiatives. Geyer, making the *Knappschaften* the central focus of his investigation, recounts their subsequent loss of distinctive characteristics and their increasing integration with other forms of social insurance. He notes the special features of mining that compounded the problems of providing workers with adequate protection. As an old industry, mining had more than its share of accumulated claims, while the labor-saving rationalization of the 1920s reduced the number of workers making contributions.

In his study of the *Knappschaften*, Geyer has chosen to pay particular attention to self-administration on the one hand and financing on the other. Under the empire, the participation of organized miners in the running of the *Knappschaften*, although limited, was important because of the refusal of mine operators otherwise to recognize union representatives. Subsequent pressures toward increased centralization and bureaucratization undermined self-administration. What remained became an early casualty of National Socialism.

The issue of financing revolved around the question of whether contributions should be high enough to cover the future claims of those making payments or whether they should serve primarily to fund current benefits, leaving later generations to support those then in need. Although advocates of social insurance over social assistance struggled to accumulate funds to cover later claims, upheavals of the magnitude of the inflation of the early 1920s and the depression of the following decade sabotaged their efforts.

Geyer's study is based on extensive research in government archives and an impressive familiarity with published materials. Although his useful notes indicate his awareness of broad interpretations and comparisons suggested by others relative to the history of German social insurance, not enough speculation finds its way into the cautious narrative. Geyer has written a book for specialists. He has provided a careful, detailed account of one

important aspect of the development of German social insurance in this century.

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ALFRED KELLY, editor. *The German Worker: Working-Class Autobiographies from the Age of Industrialization*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiii, 438. Cloth \$45.00, paper \$12.95.

By the end of the first decade of the twentieth century, writing autobiography had become increasingly common among self-conscious German workers. Some wrote their life stories for the edification of their socialist comrades. Others told all to bourgeois audiences who had become accustomed since the 1890s to buying and reading stories that recounted lives in the urban slums. The autobiographies are both products of and responses to the larger social, political, economic, and cultural transformations they record.

Obviously, as historical records such autobiographies must be used with caution. But their very subjectivity, often seen as a liability from the perspective of scientific scholarship, is also their strength. Through them we can glimpse some of the ways that people made sense of their fates and created meaning and structure in their lives. In the context of what is known about "ordinary" people of the past from anonymous reconstructions of aggregate data, we can interpret sources such as popular autobiographies as evidence of the social creation of subjectivity itself.

In this book Alfred Kelly provides a timely anthology of translated excerpts from workers' autobiographies. The anthology offers a good cross section of existing texts, in terms of both the texts' geographic distribution and the authors' occupational backgrounds. The geographic origins of the texts range from Hamburg to Bohemia, from the Rhineland to Mecklenburg (it is the language of original publication rather than the limits of the prewar German empire that qualify the autobiographies as German). The authors include factory workers, miners, agricultural day laborers, homeworkers, domestic servants, barmaids, and waiters—the broad range of men and women who could be described as working class and who indeed claimed the description for themselves.

The translations are smooth and readable. Translation is always a tricky business, all the more so from original texts that defy literary and often grammatical standards. Kelly has chosen a cautious route—his versions read roughly like the colloquial English of our day. That strategy was

probably the best for his purposes. Neither archaic nor distinctly working-class English prose would have conveyed the sense of the original, and either of those alternatives would have distracted the reader from the authors' messages.

Kelly has also provided readers with a sketchy but useful introduction to the context from which the texts emerged. There is a brief discussion of working-class life in Germany during the period of rapid industrialization and urbanization, and the editor's notes offer a few suggestions for further reading. The reader Kelly has most clearly in mind is the undergraduate student, and both the introduction and the texts are well suited to beginning scholars. Through these autobiographies, "history from below" can come alive for the student of modern European or German history.

MARY JO MAYNES
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MICHAEL RUCK. *Bollwerk gegen Hitler? Arbeiterschaft, Arbeiterbewegung und die Anfänge des Nationalsozialismus*. (Geschichte der Arbeiterbewegung; Texte-Biographien-Dokumente.) Cologne: Bund. 1988. Pp. 229. DM 29.80.

Michael Ruck's study of German labor's initial encounter with National Socialism has a twofold purpose: first, to identify the extent to which German fascism attracted working-class support during its rapid upsurge in the early 1920s and, then, to assess party and trade union responses to the emergence of a blue-collar Nazism. Ruck observes these developments with an eye on their similarities to and bearing on the events of the early 1930s. In his judgment the episode of 1922–23 established both affinities with National Socialism among certain segments of the working-class electorate and ineffectual patterns of reaction among left-wing organizations—precursors of the Nazi triumph of 1933. As he merely sketches the parallels, however, the value of his book rests on his analysis of the earlier period.

In examining Nazi gains among workers in 1922–23, Ruck notes the difficulty presented by the absence of relevant electoral data. Without numbers at hand, "qualitative" sources must provide specific information; the foremost of these are reports from local units of the Allgemeiner Deutscher Gewerkschaftsbund submitted in spring 1923, many of which appear in a concluding section of documents. The portrait they yield is of a "Nazi worker" drawn from a small-town, small-shop, nonunion Protestant milieu, a figure already familiar from earlier research. Ruck believes that this category deserved the Left's attention as "a

latent potential of [fascist] support that was rather larger than is commonly assumed" (p. 81).

In Ruck's view neither the parties nor the unions that claimed to speak for labor responded adequately to this threat. The Communists, who appraised it realistically, offered illusory revolutionism in place of action. The Social Democratic party and its union allies looked past the evidence of working-class Nazism and mistakenly relied on the Weimar state to check the extreme Right. Justified mistrust between the two Marxist parties made joint undertakings impossible.

Ruck is correct about these tactical failures, but he is not persuasive regarding their consequences. Given the indeterminacy of the statistical data and the narrow basis of his qualitative evidence, his case for "the (threatening) penetration of the working class by National Socialist and *völkisch* forces" (p. 74) is not compelling. Faulty policies among the left-wing groups did not, therefore, necessarily produce significant defections to the Nazis among workers. If such defections did occur, however, Ruck's explanation is only one alternative. The appeal of nationalism may have mattered more than Social Democratic or Communist actions; in 1923 the extreme Right drew its strongest labor support from border areas and from revolution-scarred Bavaria. A different remedy than the one Ruck indicates would have been required to stop such crossovers. Still, whatever the measure of working-class Nazism, it was not sufficient to create an "acute danger" (p. 173) to the republic and to leave the Left powerless to dispel it; this threat was posed by the hostility of the nonproletarian sectors of German society. Thus, the implication of Ruck's title and the thrust of his argument are flawed. More than the working class was needed as a "bulwark against Hitler" in 1923—and in 1933.

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SHELLEY BARANOWSKI. *The Confessing Church, Conservative Elites, and the Nazi State*. (Texts and Studies in Religion, number 28.) Lewiston, N.Y.: Edwin Mellen. 1986. Pp. 185. \$39.95.

As the most prominent expression of Protestant dissent in Nazi Germany, the Confessing church is a focal point in the vast literature on the church struggle of 1933–45. Shelley Baranowski's energetically argued essay explores this literature and takes aim at what the author sees as a common tendency, fostered in part by the dictates of cold war politics and postwar reconstruction, to portray the Confessing church as a moral sheet anchor of

that "other Germany" that allegedly refused to bow to Hitler and nurtured forward-looking liberal and humanitarian alternatives to Nazi tyranny.

In actuality, Baranowski insists, Confessing church members were part and parcel of the old Germany. Together with high civil servants and prominent professionals, landed gentry and industrial magnates, they constituted one of the influential conservative elites that initially saw National Socialism as instrumental to their own agenda for social control, and their consequent willingness to affirm the regime was more crucial in consolidating Hitler's power than the later objections were significant in deflecting its inhumane ends. If Protestant leaders resisted *Gleichschaltung* and rejected extremes of *völkisch* ideology, they did so out of a basic desire for full partnership in the Nazi order, for official recognition of their traditional social and cultural role in national life. By 1936 the Confessing front was in disarray, rendered increasingly impotent by the very forces of repression it originally helped unleash. Even the much-celebrated members of the wartime resistance, for all of their personal courage and integrity, were "the remnants of institutions more concerned with accommodation than with opposition" (p. 103).

Baranowski is clearly less concerned with providing a comprehensive history of the Confessing church than with sketching a possible heuristic framework for such a history. Her zest for generalization is coupled with thorough knowledge of the relevant scholarship; the extensive notes provide a wealth of information (and incidentally suggest that the existing literature is rather less homogeneous in outlook than the book's basic thesis might seem to imply). Baranowski makes a strong case for continuity between the Confessing church and the pre-Nazi Protestant establishment, not to mention its postwar successors. One could argue, to be sure, that she understates the degree of genuine theological reorientation that such continuity demanded in the face of Nazi doctrine and practice. It is surely worth asking, for example, why so many among the Protestant elite should have chosen to identify themselves specifically as members of a "confessing" church. Perhaps Baranowski's model of a conservative elite—stressing as it does class and institutional interests, in the manner of recent neo-Marxist scholarship on Nazi Germany—cannot fully describe ecclesiastical circumstances. Her brief references to local conditions make one curious to know more about the social reality of the Confessing church among ordinary parishioners. Surprisingly, there is no consideration of the euthanasia controversy of 1939–41, even though this issue would seem to

provide an obvious case study of her thesis about the limits of conservative opposition in the later phases of the Third Reich. Nor, for that matter, is it at all clear in what sense one can speak of the "dissolution" of the Confessing church (p. 4) after 1936.

The book offers much to argue with, as the author no doubt intended. By placing the analysis of the church struggle squarely within the framework of larger historiographical issues, the work serves as a catalyst for further discussion of questions that, as Baranowski ably documents, show no signs of exhausting their capacity to arouse controversy.

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PETER HOFFMANN. *German Resistance to Hitler*. Cambridge: Harvard University Press. 1988. Pp. 169. Cloth \$25.00, paper \$9.95.

Forty years ago, Hans Rothfels published the first general survey of the German opposition to Adolf Hitler. One of his reasons for doing that was his concern that knowledge of the existence of such opposition was very limited and that what knowledge there was appeared to him to be unbalanced. In the intervening decades, there has been a vast outpouring of literature on the subject, and Peter Hoffmann has himself contributed substantially to it. But the problems of a general ignorance of the subject and a lack of balance in what many do know remain. It is to address both of these issues that Hoffman now offers a brief survey of the resistance to Hitler inside Germany and tries to do justice to the motives and views of those involved in it.

Drawing on his own prior work as well as other publications and some German and British archival material, Hoffmann presents a general review of the issues, personalities, and difficulties of the resistance. He has set his account into a brief review of the National Socialist period, so that the general public as well as students and specialists can benefit from the book. This work is without doubt the best introduction to the subject currently available. It includes the whole range of participants, from the Communists and Social Democrats to the religious figures, conservatives, and military elements. Both because of Hoffmann's own prior work on military resisters, especially General Ludwig Beck, and because a generally supported dictatorship can be toppled only by a military coup, the emphasis is on the military opponents of the regime. Although the summary nature of the presentation does not lend itself to extensive discussion of the development of oppo-

sition over time, the author does try hard to clarify the point, which is central to his whole argument, that the resistance was not, as often argued, an opportunistic reaction to the expectation of defeat but a movement that arose out of principled revulsion against criminal policies of the regime and at times when it appeared to be winning, not losing, the war.

In a book that covers a vast subject in few pages, every reader will find some topics slighted and some points to argue with. In future editions, of which one hopes there will be many, a few errors should be corrected. Surely the German army in the East was defeated not only by the weather but also by the Red Army. The fact that foreign powers dealt with the German government cannot possibly be read as their approval of it, especially at a time when there was extensive public criticism of the Third Reich abroad. The author needs to take into account the conclusion of recent research in Germany showing that those who refused to carry out criminal orders were not seriously punished, so that this form of opposition was in fact far more readily possible than often believed. There is a general underestimation of the degree to which such Nazi policies as persecution of Jews and the murder program called euthanasia had vast support; it is now known that doctors and nurses were still murdering patients in 1945 under the noses of occupation forces. The whole question of the significance of the oath to Hitler needs further thought; the same men who claimed to think it so important were often the ones who thought nothing of breaking other oaths they took before and after that oath. The extensive discussion of the foreign contacts of the opposition needs to be brought up to date by showing that these contacts often contradicted each other, called for extensive territorial concessions to Germany, and promised an overthrow of the regime by military leaders who soon after proceeded to lead the invasions of more and more countries. If no one trusted such persons, there is no reason to be surprised. The Allies were more impressed by German deeds and policies than by promises not kept in the years when it counted.

Whatever criticisms might be made, this book should occupy a significant place in the literature on Nazi Germany. The issues are presented in a manner both comprehensive and fair. The opponents of the regime are given their due, but they are not made out to be faultless. They could not always see their way to the future clearly, and they did not always know precisely how best to accomplish their purpose. But they did have a purpose—to rid their country and the world of a horrible evil—and within their lights they attempted to accomplish it. That they were thwarted by the

massive support that the regime continued to enjoy until the end is surely the fault of the supporters, not the resistance or its chronicler.

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GOTTFRIED-KARL KINDERMANN. *Hitler's Defeat in Austria, 1933–1934: Europe's First Containment of Nazi Expansionism*. Translated by SONIA BROUGH and DAVID TAYLOR. Boulder, Colo.: Westview. 1988. Pp. xxvii, 234. \$39.95.

Gottfried-Karl Kindermann presents a picture of Chancellor Engelbert Dollfuss that differs considerably from that of the wanton “Austro-fascist” despoiler of democracy and pitiless murderer of faultless Social Democrats depicted by so many, in fact, a majority of scholars in the English- and German-speaking worlds since the fall of the first Austrian republic in 1938. In Kindermann's opinion, Dollfuss did what no European head of state had done before him: he successfully resisted Hitler's ruthless efforts to destroy Austria, and he prevented a Nazi seizure of power from within. Moreover, he redefined Austria's identity, thereby contributing appreciably to the development of an Austrian self-consciousness after the Second World War. Believing that “the ‘brown tide’ could only be kept in check if measures were taken simultaneously to force the Marxists to their knees ‘step by step’” (p. 65), the Dollfuss government, with Benito Mussolini's support, successfully fought a two-front battle against both the National Socialists and the Social Democrats that not only greatly humiliated Adolf Hitler personally but also brought about “his most painful foreign policy setback until the disasters of 1943” (p. 124). Of course, the Austrian chancellor paid a painful price for his defiance of the Führer: death by assassination on July 25, 1934.

Kindermann admits that Dollfuss's actions gave his critics some reason to complain. Among those actions were his “violations of the constitution and abuse of parliamentary rights” (p. 126). But Kindermann explains that, polarized as the Austrians were both ideologically and socially, parliamentary democracy had been destroyed long before the unexpected parliamentary debacle on March 4, 1933. Moreover, the Nazis were the only party that would have gained from the new elections repeatedly demanded by both the National Socialists and the Social Democrats. Kindermann also faults the chancellor's government for the “half-heartedness and vagueness of its approaches to the Social Democrats” and reproaches it for “the

vengeance it meted out" to the Schutzbund after the civil war of February 1934 (p. 129).

The author has based his study on many of the key sources that are available, particularly the published German, British, and U.S., but not French, diplomatic documents. He has not made much use of relevant archival sources, and he has by no means exhausted all of the published materials. Yet he should not be censured too much on this score, for he has come up with important documentation that adequately backs up his conclusions.

The British spelling and punctuation of the translation have been retained in the present volume. A cursory spot check with the original German edition indicates that the translation is quite accurate, although the translators have departed considerably from the German original in what they have italicized. In the appendix the translators have in all but two instances mistakenly used the initials ADAP, inadvertently copied from the German edition, rather than DGFP, in referring to the *Documents on German Foreign Policy 1918–1945*.

No doubt not every reader will agree with Kindermann's conclusions. I, however, find it refreshing to read a study in which the timeworn thesis of bad guy Dollfuss versus the good guys (all Social Democrats) has been replaced by a depiction of a contest between the good guy Dollfuss and the bad guys (all National Socialists).

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ELKE FRÖHLICH, editor. *Die Tagebücher von Joseph Goebbels: Sämtliche Fragmente*. Part 1, *Aufzeichnungen 1924–1941*. Volume 1, 27.6.1924–31.12.1930; volume 2, 1.1.1931–31.12.1936; volume 3, 1.1.1937–31.12.1939; volume 4, 1.1.1940–8.7.1941; *Interimsregister*. New York: Saur in cooperation with the Institut für Zeitgeschichte, Munich. 1987. Pp. cviii, 654; v, 764; v, 682; v, 741; 351. \$200.00.

The scholarly world has long awaited the definitive edition of Joseph Goebbels's diaries. Since World War II, several fragments of the diaries of the Reich propaganda minister have appeared. More often than not these sources have been unreliable, the result of commercial avarice and unprofessional editing. This regrettable situation has been corrected by the prestigious Institute for Contemporary History in Munich. The four volumes under review were edited by Elke Fröhlich, who undertook the arduous task of deciphering Goebbels's often nearly illegible handwritten notes. These volumes are but the first stage of a

ten-volume project that, when completed, will include all of the known diary fragments. Serious gaps notwithstanding—most notably the critical periods of the Röhm Putsch in 1934, the Austrian and Czech crises of 1938, and the months immediately preceding the outbreak of World War II—this collection will remain an indispensable source for serious students of the Nazi era.

The diaries shed light on many facets of politics at the highest levels of the Third Reich. Important personalities come to life, and a wealth of information emerges about Adolf Hitler's attitudes on the leading issues of the day. Readers gain new insights into the political crises of the final years of the Weimar Republic as well. Tensions within the Nazi party are clearly analyzed, and personal rivalries and turf battles take on new dimensions.

The several phases of Goebbels's career are clearly delineated. Even as a student, he was emerging as a political radical. Life for him was battle, and he came into his own only in the heat of political struggle. As Goebbels observed in September 1930, when the German crisis was reaching its peak, "It's a wonderful thing to be so hated" (vol. 1, p. 600). As gauleiter of Berlin, he developed the myth of the "fighting, dying SA man," whom he claimed represented "the steely romanticism of our era" (vol. 2, p. 76). He would later delight in orchestrating the activities of a centralized propaganda network. But he also put great store in setting the agenda for National Socialist culture through his management of the *Reichskulturkammer*. This task entailed setting policy for German film, music, art, literature, theater, press, and radio.

Trouble for Goebbels's career commenced in 1936. The mythical framework of the Nazi state had been firmly established, and his propaganda machinery was functioning well. He was bored with his wife, Magda Quandt, and he now seemed interested only in his children, luxuries, and women. When Hitler banished his mistress, the actress Lida Baarova, from Germany in 1938, Goebbels suffered a near mental collapse and was hospitalized. Only with the coming of the war did he take joy in his work again. As the Battle of France was drawing to a close, he wrote (vol. 4, p. 197): "Oh you grand German people! You have been called to reorder Europe. It is so wonderful to be living today!" The final segment breaks off in July 1941, early in the war against the Soviet Union.

The volumes devoted to Goebbels as gauleiter of Berlin during the "period of struggle" are arguably the most valuable. He fights battles on many fronts: against Alfred Rosenberg, editor of the *Völkischer Beobachter*, who jealously challenged Goebbels for control of the Nazi press; against

Gregor Strasser, a personal and ideological rival, for his betrayal of Hitler; against the jealous bureaucrats in the party directorate in Munich, whom he castigates as "alte Bierdimpfl"; against the Communists and their Comintern allies for control of the streets of the capital; against the Berlin police and Dr. Bernhard Weiss, whom he castigates as "Isidor," the stereotypical Jew; against the rebellious SA faction in Berlin under Captain Walter Stennes; against the "Jewish-Bolshevik asphalt culture" of Weimar Germany; against Chancellor Heinrich Brüning for holding the line against the NSDAP; against the "reactionaries" under Alfred Hugenberg of the German National Peoples' party and Franz Seldte, leader of the *Stahlhelm*; and against the intrigues of Chancellors Franz von Papen and Kurt von Schleicher to deny the Nazis power.

The first year of Hitler's rule offered Goebbels serious challenges. Operating from his base at the Reich Ministry for Propaganda, he details the steps he took to provide the psychological accompaniment to the armed fist of the Nazi power apparatus. His greatest satisfaction was to take part in the liquidation of the Communist threat, and he notes, like a beaming schoolboy, Hitler's approval of his work. He describes the staging of the "German May Day" of 1933, the notorious book burning, and the orchestration of the Jewish boycott. Surprisingly, readers learn of Goebbels's contempt for Germanic mystic ritual and his aversion to the work of overzealous cultural bureaucrats. Should the zealots have their way, he noted, all that would remain of German culture would be "Widukind, Henry the Lion, and Rosenberg" (vol. 3, p. 449).

Goebbels was more intimately involved in the conception, direction, and casting of German feature films than has earlier been believed, and his diaries put many legends to rest. Leni Riefenstahl was not his enemy; rather he admired her brilliant breakthroughs in film technique. And no one can again seriously question the sincerity of Goebbels's anti-Semitism. Films on "the Jewish problem" became a passion for him, and he worked on *Der ewige Jude* and *Jud Süß* for several months. Goebbels was also behind the infamous "Decadent Art Exhibit" of 1937 in Munich as well as the campaign, led by Adolf Ziegler, president of the Reich Art Chamber, to remove offensive art works from all German galleries. He worked closely with Reichsdramaturg Rainer Schlösser to coordinate the German theater, but this remained an elusive goal. The leading actor and director of the era, Gustaf Gründgens (Generalintendant der Preussischen Staatstheater), seldom pleased him. His performances were far too intellectual and nuanced to further Goebbels's propaganda goals for

the stage. It was quite the opposite with German radio, where Goebbels did not have centuries of tradition with which to contend.

The sections devoted to World War II will fascinate general readers, although specialists will be familiar with the major themes. What does emerge is a clearer image of the arrogance of power in the Nazi state. On learning from Philipp Bouhler in January 1941 that the euthanasia program was proceeding apace, Goebbels noted, "80,000 have been done away with, 60,000 yet to go. This is a difficult, but a necessary task. And it must be done now" (vol. 4, p. 485). With inhuman behavior of this nature being approved, one can better understand how the Holocaust took place.

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PETER LONGERICH. *Propagandisten im Krieg: Die Presseabteilung des Auswärtigen Amtes unter Ribbentrop*. (Studien zur Zeitgeschichte, number 33.) Munich: R. Oldenbourg. 1987. Pp. 356. DM 78.

The recent scholarly debate about the "polycratic" aspects of the Third Reich and its internal power struggles and chaotic competitiveness has persuaded many that central planning and rational decision making were rare during the Nazi regime. But such analyses have usually exempted the area of propaganda. Commentators still refer to a monolithic "propaganda machine" and Joseph Goebbels's central leadership. In a largely episodic book, Peter Longerich seeks to correct this picture by studying the competency conflicts between the Press Division of the Foreign Office and the Propaganda Ministry, and thereby illuminate the inner structure of the National Socialist regime and the roles of Führer, bureaucracy, and the ruling apparatus. He shows a remarkable command of recent scholarship and summarizes well the arguments about the "improvised" administrative programs that produced a warfare over competency and prerogatives, cloaked, naturally, behind the "Führer's will" (p. 17).

Longerich believes that the Foreign Office Press Division is an appropriate arena in which to study the influence of bureaucrats, specifically middle management (division heads and *Referenten*), on a "Führer decision" and "what consequences developed for them from the Führer's will, or put another way, how much latitude did they enjoy in engaging in conflict with other agencies" (p. 21).

The first five chapters discuss various aspects of the Foreign Office's struggle for control over foreign propaganda. The author concludes that Joachim von Ribbentrop's ideas were not sufficiently well developed to be an alternative to Adolf

Hitler's foreign program. Indeed, in contradiction to a recent interpretation, Longerich insists that Ribbentrop was psychologically and intellectually too dependent to provide serious opposition and frequently was unable to perceive or understand Hitler's long-term goals. Nevertheless, after 1941, as German foreign policy became almost exclusively foreign propaganda, Ribbentrop was in a good position to prevail against all other competitors in building and consolidating for his ministry a leading role in the creation and distribution of Nazi propaganda. This move, heavily encouraged by the ambitions of middle management officials, provides the central focus of this study.

Ultimately, the author concludes, despite perceptive criticism of Goebbels's foreign propaganda, the Foreign Office itself failed to provide any new perspective to justify the war. Thus, the propaganda line offered by the Foreign Office was almost entirely the same as that of the Propaganda Ministry, with only minor differences over tactics. Since these minor differences were hardly sufficient to cause the rash of conflicts, protests, and institutional clashes revealed by his sources, Longerich concentrates on intense personal rivalries on all levels of propaganda decision making: between Ribbentrop and Goebbels, between Goebbels and Press Chief Otto Dietrich, among all three men and the Military Propaganda Division. The heart of Longerich's presentation is a painstaking examination of why Ribbentrop was unable to capitalize on the "Führer order" of early September 1939, in which all foreign propaganda statements were to be cleared in advance with the Foreign Office. As the author shows, all the participants continued to disobey this "order," and eventually the weakness of the Foreign Office, brought about by the expansion of the war to the USSR and the entry of the United States, allowed Goebbels to assert his control. But not until 1944 could Goebbels force the Foreign Office to dissolve its press, radio, and cultural divisions. Longerich persuasively shows that, despite general agreement over propaganda lines, substantial institutional conflicts flourished as functionaries fought over their piece of turf.

Part 2 examines serially the various operations of the Foreign Office Press Division; its most interesting sections contain new documentation of the Foreign Office's attempts to gain control over wire services and, failing this, to create a wire service of its own, in competition with the Propaganda Ministry. Longerich has also integrated previously published accounts of the bitter struggle over monitoring of foreign broadcasts and for control over propaganda in the occupied and allied states. He argues that the chaotic nature of German occupation policies made it impossible for consistent propaganda to develop because Berlin

institutions repeatedly intervened in the occupied territories primarily because civil servants posted there preserved close ties to their home office, if only to keep their careers alive. Much of the material is presented anecdotally, but Longerich's evidence confirms that even the infrequent "Führer decisions" did not greatly alter the chaos.

In a final chapter the author summarizes the work of the Press Division of the Foreign Office and offers his explanation for the divisive rivalries: the initiative for the warfare against the Propaganda Ministry came from middle-management officials, often acting on their own, who forced competency disputes as ways of securing their own careers in a foreign service that after 1939 had fewer and fewer positions at home or abroad to offer its staff. This specialized study concludes with a general discussion of the role of bureaucracy and the "Führer's will," providing insightful reflections that mitigate the scattered topical nature of the book.

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UDO WENGST. *Beamtentum zwischen Reform und Tradition: Beamten-gesetzgebung in der Gründungsphase der Bundesrepublik Deutschland 1948–1953*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 84.) Düsseldorf: Droste. 1988. Pp. 328. DM 68.

In 1945 the German bureaucracy's widely held reputation for integrity and incorruptibility was, like Germany, shattered. As a gravedigger of Weimar democracy and compliant tool of the Nazis, the bureaucracy had earned world-wide opprobrium, and reform became a major goal of the victorious Allies. This was especially true for the British and Americans, who planned to reconstruct it along the lines of their own civil services. Pressure for reform began with the formation of the Bizone in 1947 and increased in intensity in 1948–49, when it became clear that a west German state would be created. These efforts failed, and Udo Wengst's richly detailed account of postwar civil service legislation in the Federal Republic shows why.

To begin with, the reforms were steadfastly opposed by the majority of German lawmakers responsible for their implementation. The reforms were unanimously rejected by the bourgeois parties, which controlled the legislative organs, and, although the Social Democrats supported some, though not all, of the Allied demands, their influence was limited. Pressure on the *Wirtschaftsrat* to pass a new civil service law was countered by German foot dragging, complaints that

such important legislation could not be rushed, and, finally, the argument that Bizon organs did not have competence to pass legislation for the impending Federal Republic. Meanwhile, the deliberations of the Parliamentary Council revealed the line that the Germans would take if left to their own devices: according to the *Grundgesetz*, future civil service legislation was to be based on past practice. Angered by German procrastination, the Allies promulgated the desired reforms by decree, increased their pressure on the Germans to act, and made future legislation subject to approval by the Allied High Commission.

The tough stand came too late. Although the legal position of the Allies was strong, their political position was not. West German civil servant interest groups were large and well organized, and their demands did not fall on deaf ears. Twenty percent of the Bundestag deputies were civil servants, and the committees that were decisive in the drafting of civil service legislation were controlled by deputies sympathetic to the demands of the interest groups. In the Bundesrat the influence of the pro-bureaucratic elements was more indirect but nonetheless effective. The governing parties were united in their opposition to substantive change, and the cabinet was overwhelmingly opposed to implementing the reforms. So was the chancellor, Konrad Adenauer, who made no secret of his support of traditional practices.

Adenauer and his legislative allies temporized, avoiding definitive action in the hopes that Allied interest would wane. Minor concessions were made, but the substantive demands of the Allies were largely ignored. A protracted dialogue developed and, as the Germans had hoped, Allied determination began to weaken. The French lost interest and, although those pushing for reform on the British side remained vigorous in their demands, their position within the British element of the high commission as a whole declined. Only the Americans persisted. A provisional law was temporarily blocked by the high commission, but this proved a pyrrhic victory. The tide was shifting in favor of the Germans, who were now able to play the trump card of rearmament. The revision of the Occupation Statute in 1952 completed the erosion of the Allied position. As a result, the *Bundesbeamtengesetz*, finally passed in 1953, bore few traces of the reforms demanded by the Allies.

Wengst's account of this complex chapter in postwar German history is a model legislative history. He has thoroughly used the materials in the relevant parliamentary, ministerial, party, and organizational archives. In addition to tracing the history of the *Bundesbeamtengesetz*, he includes a lengthy and welcome chapter on the legislation regulating benefits for civil servants who had lost

their positions as a result of Germany's territorial losses or denazification.

The failure of efforts to reform the bureaucracy is often cited as a prime example of the "restoration" of authoritarian, undemocratic institutions in the Federal Republic. Wengst argues that the term "restoration" is anachronistic, since the contemporary terms of debate were—as his title indicates—tradition versus reform. Although conceding that postwar civil service legislation contained much tradition and little reform, Wengst concludes, rightly, that one cannot speak categorically of a restoration. Much remained the same, but there were important changes. Postwar legislation may have ensured too great a degree of continuity, but it also helped forge the bureaucracy's loyalty to the new democratic state. Thus, the Weimar analogy does not apply to Bonn. Indeed, the rejection of Allied demands and the successful defense of what were held to be sound German traditions (also evident in the shaping of postwar legislation on social security and war victims' benefits) may well have helped legitimize the Federal Republic in the eyes of many of its citizens.

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KARSTEN SCHRÖDER. *Die FDP in der britischen Besatzungszone 1946–1948: Ein Beitrag zur Organisationsstruktur der Liberalen im Nachkriegsdeutschland.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 77.) Düsseldorf: Droste. 1985. Pp. 329.

This book bears the hallmarks of thorough scholarship: wide use of archival material and secondary works. Karsten Schröder's approach, to tackle the organizational structure of the Freie Demokratische Partei (FDP) at the highest level, that is, that of the zone, is perhaps the only practical one in view not only of the notorious fragmentation of liberalism in Germany throughout the country's political history but also of the almost total dislocation in Germany during the immediate postwar period. Moreover, the concentration on the organizational framework excludes any "ideological or philosophical critique" of the party, which the author rejects in favor of the "historical-descriptive method." This focus results in a sober account of structures and decision-making processes and allows Schröder to trace the enormous problems the party faced.

Unlike the other parties, the FDP could not use previously existing structures for rebuilding. It had to overcome the historic split of liberalism into "national-liberal" and "democratic" wings, a process that ended with the predominance of the

national-liberal tendency as the determining factor in the later FDP in the Federal Republic. There was also an inherent imbalance between the aspiring zonal headquarters and one very powerful regional organization, that of North Rhine-Westphalia. This imbalance became particularly important for the question of finance when regional organizations were reluctant to pass on membership fees to the center. The FDP leadership also felt that the military government and the German authorities treated the party less than fairly.

The shortest section of the book is devoted to the party's economic and social programs, although it seems that here interesting parallels between the FDP and the Christlich Demokratische Union (CDU) emerge. In both parties the commitment to policies of economic fairness and social justice was relatively strong in the immediate postwar years. Successive party conferences showed, however, the increasing weight of the capitalist wing in both parties. For the FDP this process culminated in the Wangeroog Program of 1948 and a firm commitment to a market economy. This goal was to some extent carried over into social policy when the party rejected one national insurance plan in favor of a private insurance scheme. Indeed, the party found it hard to formulate any cohesive social policy. During the period under discussion, only two contrasting documents emerged, and no final official program was worked out. The documents were produced by the zonal Social Policy Committee and the regional party of North Rhine-Westphalia.

Unfortunately, Schröder deals only cursorily with the actual programs and thus misses an opportunity to place the internal party debates in a wider framework of policy discussions of the time. It seems that the limitation of a strictly organizational approach appeared safe to the author where the wider treatment of policy issues would have necessitated a more ideological treatment. This issue applies particularly to the ambiguous attitude of many FDP members toward National Socialism, which is completely excluded from this study. National Socialism greatly affected the new party, and inclusion of it would have been beneficial even to a book with its main focus on party structures.

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WAYNE A. REBHORN. *Foxes and Lions: Machiavelli's Confidence Men*. Ithaca: Cornell University Press. 1988. Pp. xiii, 267. \$24.95.

Wayne A. Rebhorn's argument is that the confidence man is the central figure in Machiavelli's

thought; that his perpetual presence bifurcates the social world into tricksters and dupes; that all of life, in consequence, is a confidence game and every person, wittingly or unwittingly, a player; that defeat can be fended off for a while but not forever; and that however inevitable defeat may be, it always invites the conclusion that the loser was a dupe. What matters, therefore, is how well we play the game, how many victories we amass before the final defeat, and how well we learn, through irony, to turn defeat itself into something permitting us to salvage a modicum of self-respect. Rebhorn's comprehensive and thematic presentation draws on the entire corpus of Machiavelli's writings and makes considerable use of the secondary literature. Cleverly organized, gracefully written, and informed by a fine sense of the nuances of the Italian language, his book grapples with central concerns of Machiavelli—and Renaissance—studies.

Much of what Rebhorn is up to revolves around his contention that literature does more than record a pre-existing reality; rather, literature constructs reality. All of Machiavelli's experiences were shaped and molded by the mental categories present in the Italian and classical texts that he knew so well. Tricksters were the very stuff of Boccaccio and Plautus, hence of *Mandragola*, and even of Machiavelli's personal experiences as recounted in his letters. When writing history, Machiavelli instinctively invoked the epic genre, adding the innovation of depicting a political world in which the trickster is as central to what is "real" and serious as he is to what is fictional and comic. All the world's a stage and the confidence man the one actor who never exits.

Rebhorn also sees Machiavelli as something of a confidence man in his relationship to the Medici and to his larger audience. Forced by humble birth into the role of servant, Machiavelli devised literary strategies in a bid to master his unworthy masters. Ultimately and inevitably, Machiavelli was unsuccessful in his attempt to make the Medici and his audience listen. As a way of rescuing his self-image, as a means of escaping from the deeply damaging conclusion that defeat had turned him into a dupe, Machiavelli resorted to a brilliant use of irony. Less than successful even in his efforts to hoodwink the friars at Carpi, Machiavelli managed to rise above his embarrassment by turning a minor misadventure into a first-rate comedy (chapter 6).

Rebhorn's interpretations are, of course, not above criticism. Most readers, I suspect, will regard as strained his view (chapter 2) that Ligurio will vie with Callimaco for the bed of Lucrezia after the curtain falls on the last act of *Mandragola*. Only Hanna Pitkin is likely to agree that there is

sufficient evidence to warrant the claim that for Machiavelli *fortuna* is a mother figure who threatens to reduce the hero to the status of a child (chapter 4). Why Rebhorn takes literally and draws serious psychological consequences from Machiavelli's humorous letter about a sexual encounter with an old hag (chapter 6) is also far from clear; Machiavelli loved a tall tale, especially one that outdid that of his correspondent. The one point at which Rebhorn's argument is seriously deficient concerns his essay on Machiavelli the servant who through rhetorical tricks would be master of his readers (chapter 5). To say that Machiavelli wished to dominate the reader with fox-like guile and lion-like force is to borrow a metaphor; to ask such a borrowing to carry the weight of the argument is to permit a metaphor to run riot. All authors seek to persuade, but that does not entitle us to call them confidence men.

In his first chapter, Rebhorn follows those scholars who use modernization theory to discover the birth of individualism in the Renaissance. As an advocate of the power of literary genres to structure experience, Rebhorn might do well to reconsider this revival of Jakob Burckhardt. Is Machiavelli's Cesare Borgia really an individual, or is he one more of those idealized types who characterize humanist literature, the ideal power-politician taking his place beside Castiglione's ideal courtier and Cicero's ideal orator?

It is some measure of the quality of this book that, after rejecting this or that part, I find the central structure still standing as an unusually rewarding study.

MARK HULLIUNG
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MARK PHILLIPS. *The Memoir of Marco Parenti: A Life in Medici Florence*. Princeton: Princeton University Press. 1987. Pp. xiv, 283. Cloth \$40.00, paper \$9.95.

Until Mark Phillips established that Marco Parenti authored an untitled memoir about events in the 1460s, historians knew Parenti mainly as Filippo Strozzi's brother-in-law, the man who helped Alessandra Macinghi Strozzi watch over her two sons' interests during their exile in Naples. Now Phillips has brought the memoir together with two other kinds of historical records concerning Parenti, his and Alessandra's correspondence with Filippo Strozzi and a manuscript of household *ricordanze*. The result is a composite, if fragmentary, picture of Parenti's public and private life in a particularly interesting period of Florentine history during the anti-Medici conspiracy following the death of Cosimo de' Medici in 1464. Parenti

becomes the protagonist in a drama in which historically he played a bit part. He was not a member of the Medici faction, nor was he intimate with the conspirators. But fascination with him lies in his position as witness on the sidelines, for he made very acute observations.

Parenti came from a patrician family and acquired a good education, but, contrary to the expected Florentine pattern, he associated more with his wife's family than with his own. He had married above himself and was quickly drawn into the Strozzi orbit. Whether by design or by happenstance, he created a valuable role for himself as Filippo Strozzi's surrogate in Florence, which thrust Parenti into contact with the most powerful people in Florence. As his later memoirs show, he kept his eyes and ears open.

The more general problem addressed by the book concerns the relationship between the three kinds of historical writing that Parenti left, which culminated in the memoir. Phillips views the memoir as a successful combination of two strands in Florentine historiography, Giovanni Villani's style of vernacular chronicle and Leonardo Bruni's kind of rhetorical history inspired by antiquity. As such, Parenti's writing foreshadows the histories of Niccolò Machiavelli and Francesco Guicciardini. Parenti's memoir also contains long discursive passages on events outside of Florence, much garnered, one supposes, from information Filippo Strozzi sent him from Naples. Phillips treats this aspect of the memoir only in passing and concentrates, instead, on unraveling Parenti's reflections on contemporary Florentine history, where the rhetorical thread is more apparent.

For Phillips, the central dilemma behind Parenti's memoir concerns the inadequacies and failures of patrician politics, which permitted the Medici to assume ascendancy in Florence. Civic traditions gave way to personal interests, and he concludes that Medici hegemony forced the Florentine patricians to reevaluate the boundaries between private and public. Of course, Parenti's vantage point circumscribed his view of Florentine politics. He had little appreciation for the subtleties of Medici government and how Cosimo and Piero, and soon Lorenzo, purposefully restrained their power to maintain civic forms. The Medici did not limit the patriciate's role in government so much as redefine it by inserting themselves behind the scenes to control officeholding.

The book contains some technical problems. The index is spotty and, therefore, misleading. Some references to such key figures as Piero de' Medici are missing, and pages of excerpts from the memoir are not indexed at all. The author translated long sections of the manuscript into English, and one wishes he had provided the

original, unpublished text in either the notes or an appendix. Folio references do not correspond to the numbers in the manuscript. Despite these scholarly frustrations, Phillips has crafted a sensitive and illuminating study that greatly enhances our understanding of *quattrocento* Florence.

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DENNIS ROMANO. *Patricians and Popolani: The Social Foundations of the Venetian Renaissance State*. Baltimore: John Hopkins University Press. 1987. Pp. xiv, 220. \$25.00.

While the city-states of northern and central Italy experienced factional strife and rebellion during the fourteenth century, the Venetian oligarchy maintained political and social control with remarkable success. Dennis Romano seeks to broaden our understanding of this stability by going beyond the explanations of renowned historians such as Frederic Lane, Alberto Tenenti, and Roberto Cessi. Romano sets out to analyze "how individual Venetians reacted to and interacted with one another," to study "the city as the people" as "a network of networks" (p. 9).

Through his examination of numerous and previously untapped sources in Venetian archives—guild records, marriage contracts, testaments, and governmental grants of pardon (*grazie*)—he argues that the major rift in Venetian society was not between the ruling elite (who, by the legislation of 1297, had become a *de jure* nobility) and the people but between rich *popolani* and nobles, on the one hand, and the rest of the population, on the other. The rich, noble or not, intermarried and relied on extended kinship relationships, while, for the poor, the conjugal household was the only source of support and intimacy. They lacked extended kinship networks and neighborhood and occupational stability.

Romano's approach to the question of Venetian stability, however, yields a meager harvest. His solution to the riddle of Venetian stability amounts mostly to a melding together of earlier theses: stability depended on the strength and solidarity of the patriciate (albeit with the cooperation of upwardly mobile *popolani*) and on the "alienation" (p. 147) and dissolution of the workers (*popolani minuti*). Positions in the *scuole piccole* (the religious confraternities) and charity from the grandees further lubricated the wheels of stability.

Methodologically Romano has turned the clock backward. In place of statistics, he uses examples as the means for justifying claims about social structure and social interaction. Two examples,

for instance, underlie his claims about the complexity of kinship networks among the nobility. In some cases, no examples, no notes to other sources, no statistics appear. He claims that many *popolani* widows, presumably unlike wealthier women, remarried (p. 62). The "people" lacked permanent homes (p. 86). Venetian converts were the preserve solely of the elites; the fees were too high for artisans (p. 104). Where statistics do appear, Romano's interpretations of them are troubling. Certain figures appear as strong indexes of social phenomena, while others, which are much larger, do not. For instance, Romano's claim that the nobility and the wealthy *popolani* composed a single community of like interests and behavior rests in part on evidence of intermarriage between the two groups. But, once the examples have been sifted through, we find that the nobility intermarried with non-nobles in only 5.6 percent of the cases (p. 50). On the other hand, he asserts that workers had little sense of occupational solidarity in part because sons rarely followed in their fathers' footsteps—in "only" 52 percent of the cases (p. 83). These interpretations might have been convincing had these statistics been compared with those from other societies.

In conclusion, Romano made inroads into untapped archival sources to confront a problem of central importance for late medieval Italian history. The results, unfortunately, are confusing and disappointing. His final pages maintain that Venetian stability during the Trecento rested on "the ideal of community" (whose?), while only seven pages earlier, he emphasizes the "alienation" and "isolation" (p. 147) of workers, and the body of his work stresses the difference between men's and women's "systems of patronage" and the contrasting worlds between the wealthy and the poor.

SAMUEL COHN, JR.
Brandeis University

JOHN DAY. *Uomini e terre nella Sardegna coloniale XII-XVIII secolo*. (Cultura materiale tecniche economiche società insediamenti, number 3.) Torino: CELID. 1987. Pp. 338. L. 38,000.

Several years ago a reviewer of a magisterial two-volume history of Sicily voiced his admiration for the authors who, in the face of the island's mighty tragedies and severe cultural and economic declines, yet retained their vitality and completed a monumental study. Something of the same compliment should be paid to John Day, whose economic and social history of the benighted island of Sardinia from the twelfth to the eighteenth centuries is a brave enterprise. He is

well qualified for the task having published several studies on the island's history, which are included in this volume.

Day informs the reader that Sardinia can serve as a laboratory for the study of the many ills perpetrated first by the Genoese and Pisans, then by the Aragonese, the Catalans, the Spanish crown, Turkish pirates, and the rest. He goes lightly on the Sardinian aristocracy, a point to which I shall return. Clearly, the Sardinian experience does not fit any European model of economic progress. Not progress but survival was at stake in this colonial dependency! No Malthusian scissors or Ricardian law can be invoked as an explanatory principle in this land of underpopulation and deserted villages. Wages and prices, according to Day, obeyed quite different norms from those of mainland Tuscany. An insufficient money supply and harsh fiscal exactions combined to depress an already burdened economy. Cruel Aragonese rule provoked resistance and rebellion and, in turn, stern countermeasures. Again, native insurrection and civil war ensued. Pacification was achieved by Aragon; local nobles were invested with great power. After the mid-fifteenth century, this local aristocracy bears much of the blame for the economic decline of Sardinia, and here Day could have been much harsher. His chapter on Sardinian bandits will be of general interest to readers who have been nourished by Marxist theorists such as Antonio Gramsci and the views of Eric Hobsbawm in the 1960s. Day's chapter on criminality and lawlessness indicates that Fernand Braudel's grand ideas on Mediterranean culture will bite the Sardinian dust. It should be pointed out that, for the period treated by Day, bandits were just as likely to place their violent talents at the disposal of rich landlords in the landlords' struggle against royal administrators or impoverished peasants as the bandits were to serve in the heroic mission assigned them by romantic modern-day historians.

The author's range is admirable, and he is well informed about Sardinian history beyond the temporal bounds advertised in the title of his book. He also incorporates his findings into the framework of the general debate of medieval economic historians concerned with the fourteenth-century crisis. His method is that of scholars of the *Annales* school, and he shares in their many successes. His shortcomings are also theirs—a too abstract view of society and a tendency toward quantification, even when the data are suspect and the inferences drawn are trivial. On balance, however, this is a serious and fundamental contribution to our understanding of the demography, the pattern of settlement, and the many other economic and

social features of the history of this troubled island.

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LUCIANO ALLEGRA. *La città verticale: Usurai, mercanti e tessitori nella Chieri del cinquecento*. Milan: Franco Angeli. 1987. Pp. 235. L. 24,000.

Although local and regional studies abound for the late Middle Ages, the Renaissance, and the modern period in Italy, they are still relatively rare for the early modern period. Thus, Luciano Allegra's study of the small "agro-town" of Chieri in the region of Turin during the sixteenth century should be welcomed. Yet this study does not succeed in deepening or extending our understanding of the period.

The title, *The Vertical City*, succinctly expresses Allegra's overriding thesis, that is, solidarities, which extended from one class to another, were the principal bonds of society in Chieri; they were stronger than class alliances. This study opens with a brief consideration of the population of the town. Despite the claim of his introduction that Chieri "changed profoundly" over the course of the *cinquecento* from a populous to a sleepy minor center (p. 16), Allegra's demographic estimates show Chieri at the beginning of the seventeenth century with the same population that it had at the beginning of the previous century, around ten thousand.

Part 1 of the work looks at usury and the relations between creditors and debtors. Allegra finds an inverse relation between the social distance that separated creditors and debtors and the extent of violence between them. But, like so many of the claims in this book, Allegra does not go beyond examples. Statistical comparisons over time are shunned. Part 2, the most interesting section of this book, examines the economic development of Chieri from cotton to fustians and the growth of "protoindustry" (p. 99) in which the city population of weavers and other cloth workers divided its labor between manufacturing and agriculture. Yet, from Allegra's description, it is not clear when or why this transformation took place. The final section introduces the political dimension—the transformation of power from French occupation to incorporation into the Duchy of Savoy in 1562. According to Allegra, the "new state" of Emanuele Filiberto decisively changed Chieri society, creating "a new sense of social conflict" (p. 181). But what was this new *confittualità sociale*? The vertical city merely became more vertical, that is, intra-class, or horizon-

tal, conflict was accentuated as vertical solidarities between classes were reinforced. Again we are left only with examples and Allegra's claims. Although he has compiled criminal statistics (pp. 220, 229), he does not bother to break them down chronologically. He fails to examine historical change systematically.

Despite the claims of his introduction and a few pages describing the political events of the second half of the century, this study lacks historical and comparative dimensions. Usury, honor, and violence are discussed as timeless historical blocks without systematic comparison over time or across geography. Allegra does not even mention in his text the now-classical study of Lucca during the same period by Marino Berengo. (There is one reference in the notes.) In one of the few instances when Allegra ventures beyond the walls of Chieri, he fails to consider the implications of his comparative materials. He finds that the degree of intra-class crime in this city was little different from that of Florence during the period of the Ciompi. But he fails to ponder the implications of this comparison. Although Allegra uses the high rates of intra-class crime to substantiate his thesis—the "verticality" of his city—the criminal statistics from the larger and more revolutionary Florence of the fourteenth century show that crime rates between members of the same class vary directly with increases in class tensions.

In conclusion, Allegra's examination of Chieri strives to be more than just another local study. He has intended "to reconstruct the social morphology of the city" (p. 17). The reader leaves this work, however, with little sense of place and little sense of the historical specificity of the sixteenth century in northern Italy.

SAMUEL COHN, JR.
Brandeis University

GIOVANNI LEVI. *Inheriting Power: The Story of an Exorcist*. Translated by LYDIA G. COCHRANE. Chicago: University of Chicago Press. 1988. Pp. xviii, 209. Cloth \$29.95, paper \$12.95.

Do not be misled by the title of this book; anyone wishing to learn about exorcisms from this study receives short shrift. The real protagonist of Giovanni Levi's sensitive investigation is the seventeenth-century Piedmontese village of Santena, twelve miles southeast of Turin. Several noble houses (two of which divided one twenty-fourth of Santena's jurisdiction between them) maintained castles here while pursuing family survival strategies in the capital. Meanwhile, clans of estate sharecroppers (*massari*) and other local notables exchanged innumerable tiny parcels of land in

accordance with complicated extended-family strategies. From 1647 to 1690, a landless immigrant judge (*podestà*) and notary named Giulio Cesare Chiesa mediated all disputes. His eldest son, installed as Santena's vicar, faced Turin's archiepiscopal court in 1694 on charges of professional malfeasance. Three years later he undertook a traveling campaign of exorcisms, including a few dozen in his native village, for which the Turin Inquisition investigated him.

Levi attempts a "total history" of Santena from 1672 to 1709; he accumulated thirty-two thousand note cards on a small village of a few hundred people. He uses them to slaughter two sacred cows of social history, both of British origin, with a fine Italian rapier. First, the coresident nuclear family beloved by English demographers was not a coherent unit of social strategy in seventeenth-century Piedmont. At Santena, Levi must "speak of families in the sense of non-coresident groups of people nevertheless connected by ties of consanguineous kinship, of alliance, or of fictitious kinship; families that appear, in the nebulous institutional world of the ancien régime, as wedge-like structures designed for self-assertion amid the incertitudes of the social arena of even a tiny village" (p. 38).

The second dead cow is the economic rationalism of peasant land sales. Santena's peasants incessantly sold small parcels of land to each other and to strangers but at prices that bore little relationship to their actual worth: lands of equivalent value sold at anywhere from twenty to five hundred lire per *giornata*. Even agents of the duke of Savoy found that sale price was useless as a guide to land value (pp. 86–87). Levi's important law of peasant land sales, beautifully demonstrated by graph 3 on page 90, claims that the closer the buyer, the higher the price.

Levi's portrait of Giulio Cesare Chiesa dominates that of his luckless son. The elder Chiesa was a truly liminal personage; married to a bastard daughter of Santena's leading noble clan, he managed to make himself disappear from the duke's tax rolls through a precocious example of creative accounting. Using an old law exempting parents of twelve children from all taxation, Chiesa joined his five with the children of two noble households (one had five, the other two) to claim an exemption that remained unchallenged for decades (pp. 118–19).

Levi's Santena is an interesting but oddly indeterminate place. Its legal boundaries were deliberately imprecise; its seventeenth-century population was never counted, even by a visiting archbishop; even the exact legal status of its vicar, the exorcist, was unclear. Fittingly, all of the legal records of *podestà* Chiesa were destroyed by

French troops in 1691, and no traces of his son, the *signor vicario*, can be found after 1697 (pp. 119, 19). Amid this haze, only the social strategies of its various extended families emerge clearly in Lydia Cochrane's fine translation.

WILLIAM MONTER
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MARINA ROGGERO. *Il sapere e la virtù: Stato, università e professioni nel Piemonte tra settecento ed ottocento*. (Deputazione subalpina di storia patria, miscellanea di storia italiana, series 5, number 1.) Torino: Palazzo Carignano. 1987. Pp. xi, 244.

This work is an institutional study of the Provincial University of Turin (Collegio delle Province), founded by the king of Sardinia in 1739. Unlike openings at the seventeenth-century Savoyard university, which were for the nobility, spaces in the Provincial University were for scholarship students and *pensionanti* (those who paid minimal fees for lodging, board, and books). Admission was determined by "poverty, moral comportment and talent" (p. 45). Unlike the Jesuit colleges, service to the state instead of to the church was the ultimate goal of the university's founders.

In contrast to Harvey Chisick's conclusions in his study of French universities in the *ancien régime*, Marina Roggero, in the preface to this book, proposes that the Provincial University did modify the social hierarchy of the Old Regime. Without breaking down the old order, the university "accompanied a process of social mobility, which favored modernization," by providing those from humble backgrounds entry into the ranks of the "minor professions" (p. ix). Larger questions regarding the social and political impact of this institution, however, are lost in the detailed and well-documented chapters on daily administration, the recruitment of students, and the careers of its graduates. Only the final chapter examines historical change. In the Napoleonic period, the original charter of the university was broken. It no longer served a student body supported mostly by free scholarships (even if from the beginning those from privileged backgrounds shared in these educational spoils); admissions changed from criteria based on "piety and good comportment" to "proof of intelligence and Republican virtues" (p. 183); and recruitment was extended nationally, with only a fourth of the positions reserved for natives of Piedmont and Savoy. Finally, the university became better geared for the purposes of the new French state. Ideological indoctrination was introduced through courses on the laws and acts of the government; places for theology students were suppressed; and students

enrolled in the new discipline of veterinary science.

This book will serve well the series, "Studies and Sources for the History of the University of Turin," in which it was published. It also will provide materials for those interested more generally in the history of higher education in early modern Europe. But, because it fails to draw concrete and precise conclusions about the university's impact on larger social and cultural problems, the work deserves only a limited readership.

SAMUEL COHN, JR.
Brandeis University

PIER GIORGIO ZUNINO. *L'ideologia del fascismo: Miti, credenze e valori nella stabilizzazione del regime*. (Saggi, number 287.) Bologna: Mulino. 1985. Pp. 429. L. 30,000.

How did Italians under Fascism conceive of the words "liberty" and "dictatorship"? How did they relate Fascism to the Risorgimento? What was their image of the United States and the Soviet Union?

Pier Giorgio Zunino's study explores some of these fascinating questions in his probe of the *mentalité* of Italy in the 1920s and early 1930s. To do so, he has bravely plowed through the secondary literature of the period and the provincial and second-rate newspapers, magazines, and journals. In six chapters, he investigates what Italians thought of time and history, state and society, the "new economy," and the international order.

His study is an interesting evocation of an epoch, a journey back into the way it really was—at least in the mind. For example, he reminds us of the powerful streak of anti-Americanism under Fascism—a stance that was "comic" before it was "stupid," Antonio Gramsci noted (p. 324). The United States that Italians saw during the 1920s and 1930s was an "industrial Moloch," a "pseudodemocracy" that was doomed to collapse because it had no spiritual anchor or traditions beyond money. "Too much wealth dries up the brain," noted one commentator (p. 327). Ironically, Zunino points out, anti-Americanism was one of the ideological strains that led Fascists and Communists to converge in their opposition to the democracy and plutocracy of the West.

Given his broad approach, the author's conclusions are not surprising. Fascism was anything but the granite monolith that the regime pretended that it was. In its way, it was a system of "checks and balances" (p. 371), a "pluralist power," a series of compromises that had to be negotiated day by day (p. 372). There was no single "fascism" but

many "variegated fascisms" (p. 374). Zunino remarks.

Other conclusions are startling and sobering, particularly when the author deals with the regime's collapse. Fascism, he argues, had ideological substance. If it was only a superficial covering or a Crocean "parenthesis" to the nation's real sentiments and traditions, then the year 1943 should have been the year of "antifascism," he argues (p. 390). Such was not the case, according to Zunino. Italy did not "demolish" Fascism; Italians did not freely cease to believe in it; they were "forced to stop believing in Fascism" (p. 393). Thus, Italy did not ascend from tyranny to democracy; it "slipped or slithered" (*sdruciolasse*) into democracy, concludes Zunino.

Despite its intrinsic interest, any investigation of this sort presents some major methodological problems that Zunino does not always resolve satisfactorily. Who were the obscure authors and editorial writers whose work Zunino examines? Conscious of this problem, he does provide some brief biographical sketches at the end of his work, but for the most part they are too skimpy. More important, how representative are the writers whom Zunino cites? Although he claims to have gone beyond the elite of the regime, at best he has only enlarged the circle a bit.

Another difficulty is the contrast between beliefs and reality. Too often, the book becomes "abstract," as the author himself admits, a catalogue, a string of quotations. Zunino needs to tell us not only what people thought and wrote but also what the realities were. For example, he describes how the Fascists idolized Francesco Crispi as a great precursor of the regime. He needs to remind us briefly whether the Fascists were right or wrong in making a hero out of him. Would Crispi have admired Mussolini and his regime?

Finally, there is the problem of Zunino's time frame. He limits himself approximately to the period from 1920 to 1935. But why stop there? What happens during the latter years of the regime? Is he implying that the "real fascism" ended about the time of the Ethiopian war? If so, why?

Despite these shortcomings, despite an excessively florid style and some sloppy editing, this is an interesting step in a line of research that deserves to be further developed.

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ANDRÁS SIKLÓS. *Revolution in Hungary and the Dissolution of the Multinational State, 1918*. (Studia

Historica, number 189.) Budapest: Akadémiai Kiadó. 1988. Pp. 172. \$18.00.

This brief publication is a less than precise translation of the second part of András Siklós's doctoral dissertation, *A Habsburg-birodalom felbomlása 1918: A magyarországi forradalom* (The Dissolution of the Habsburg Empire, 1918: The Hungarian Revolution), published in Budapest in 1987. As do most historians of 1918–19, Siklós sees the revolution as a consequence of social and national conflicts that were exacerbated by World War I. Military defeat led to the collapse of the Habsburg monarchy and to revolutions among the nationalities of the multinational empire.

The dissolution of multinational Hungary, which took place during the liberal-democratic Hungarian revolution of October 31–March 21, 1919, is the major focus of the book. The author sees the dismantling of Hungary as a consequence of a French foreign policy hostile toward Hungary and the aggressive policy practiced by some of Hungary's neighbors—Czechoslovakia, Romania, and Serbia—that were identified with the victors. The description of some of the activities of various national councils of different nationalities that sprang up in Hungary is a welcome contribution to the English-language historiography of the revolution. But the doctrinaire Marxist analysis, absent in most other recent Hungarian publications on history, distracts from the value of Siklós's effort. Thus, the author claims that the national councils, dominated by middle-class elements, favored secession because the national idea diffused socialist revolutionary goals and succeeded in its ability to "disrupt the unity among workers of different nationality" (p. 145). The revolutionary government of Mihály Károlyi is also seen as a bourgeois institution. It "did not wish to topple bourgeois order. On the contrary, by eliminating remnants of feudalism, it wished to create a bourgeois Hungary to strengthen capitalism via reforms" (p. 55).

The looting and lawlessness that accompany revolutions because of the breakdown of order were also evident in Hungary and are uncritically identified by the author as being revolutionary expressions of the mass movement, which were repressed (p. 61). The author claims that in these efforts to crush popular revolutionary actions, the Hungarian, German, Slovak, and Romanian bourgeoisie joined forces (pp. 103, 111). Because the author sees struggle and deadlock (and not a "balance of power," as it is translated into English) between bourgeois and popular revolutionary forces (p. 61), an inordinate amount of space is dedicated to the Communist movement in Hungary. The role of Magyar and non-Magyar Com-

munists is exaggerated; so is their number, put at thirty to forty thousand (p. 135). Western scholars estimate their number to have been between two and four thousand.

Siklós also suggests that, had the popular revolutionary forces—that is, the Communists—won the day, dismemberment of Hungary would not have taken place. Expressions of support for the integrity of Hungary by Slovak and Romanian Communists and their publications are marshaled by the author (p. 138) to back his view. These claims of internationalism may be part of Marxist-Leninist theory but cannot be proven by the thoroughly examined historical evidence. In sum, the editor of the series would have done better had he chosen to publish an English-language monograph written by any one of the other Hungarian specialists of imperial collapse and revolution, among them, Mária Ormos, Tibor Hajdu, and György Borsányi.

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ANDRZEJ GARLICKI. *Od Brześcia do maja* [From Brest to May]. Warsaw: Czytelnik. 1986. Pp. 314. 250 Zł.

This book is the fourth in a series by Andrzej Garlicki devoted to an examination of the personal role of Marshal Józef Piłsudski in the shaping of interwar Poland. From the beginning of this project, Garlicki has attacked the Piłsudski legend with an almost missionary zeal. The current work, dealing with the last five years of Piłsudski's life, is bound to provoke considerable controversy. Here the mask of legend is completely removed, and the resulting portrait of the historical personality behind it is most unflattering.

The book begins with the arrests in September 1930 of twenty leaders of the parliamentary opposition to Piłsudski's *Sanacja* regime and their subsequent internment at the military fortress of Brześć in eastern Poland. The arrests, ordered by Piłsudski to break a constitutional deadlock with the Polish parliament, were followed by new "manipulated" elections that ensured the government a parliamentary majority. In subsequent years the government forced through individual acts of legislation that created a legal basis for "an efficient system of repression" (p. 119). The dismantling of Polish democracy was crowned, according to the author, by the "anti-egalitarian" (p. 280) constitution of April 1935, which dramatically strengthened state authority while simultaneously undermining the principle of universal suffrage, the competence of representative bodies, and individual civil rights.

In this dictatorial system, Piłsudski "controlled

all important political decisions" (p. 216) and is therefore deemed personally responsible for its successes and failures. Hard-pressed to find any successes, Garlicki concentrates exclusively on failures. Lacking a practical program, Piłsudski and his principal lieutenants failed, according to the author, to resolve any of the critical political, social, and economic issues facing Poland, including the cardinal problem of the country's security between two hostile neighbors.

Even more damning is the author's psychological portrait of Piłsudski in his last years. Already at the time of the Brześć affair, Piłsudski is presented to the reader as lonely, embittered, and self-centered, a near recluse increasingly preoccupied with his own death. As his health deteriorated, Piłsudski grew exceedingly aloof from affairs of state, which did not, however, prevent whimsical interventions that disorganized the work of subordinates. Toward the end of his life, periods of intellectual clarity alternated with disorientation, light moments of reflection with uncontrollable fits of rage. The charismatic leader had become old, sick, and pathetic. Nevertheless, he preserved his authority among a leading group that was unable to fend for itself. In Garlicki's account, Poland became the real victim of the marshal's sclerosis.

In his conclusion Garlicki calls Piłsudski a "great man" who left his mark on Polish history for several decades, a legend in his own lifetime, and a symbol of the country's resurrection. Yet in the final analysis the "great man" suffered defeat as a result of his inability to realize his vision of the state. An anachronistic dictator who did not understand the requirements of the times, Piłsudski "left behind a state that was politically fragmented, with endangered borders, a backward economy and a weak army" (p. 304).

In his conscious demolition of the Piłsudski legend, however, Garlicki may have created one of his own. Too frequently the author adopts the polemical language of the "democratic" opposition to the *Sanacja* regime rather than that of a detached historian. Piłsudski's inability to solve the great problems confronting interwar Poland, moreover, was hardly a personal failure, since those problems essentially defied solution, especially by one individual in the course of a few years. Finally, all legends are based on certain realities. Piłsudski was more than just a symbol of the country's resurrection. He was its living embodiment and, during the Polish-Soviet war, its principal defender. Without the reality of Piłsudski's lifetime devotion to the cause of Polish independence, there would be no legend for Garlicki to attack. The task of the historian is neither to destroy nor to promote legends but to under-

stand and explain their essence. It is this kind of sensitive analysis that is missing in Garlicki's book.

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DAVID S. MASON. *Public Opinion and Political Change in Poland, 1980–1982*. (Soviet and East European Studies.) New York: Cambridge University Press. 1985. Pp. xii, 275.

Crisis situations afford us glimpses into closed societies. When central authorities no longer completely control public behavior and the media, private thinking is revealed. Such situations arose in Czechoslovakia during 1968 and in Poland during 1980–81. In both cases official myths soon papered over underlying realities, though not very effectively in Poland.

David S. Mason argues convincingly that the events of 1980–81 in Poland provide us with a rare opportunity to study Eastern European politics "from below" and to do so, in part, by making use of unusually reliable data gathered in opinion polls. Mason claims that public opinion's influence on Eastern European public policy has grown in recent years. Whereas it once played only a supportive role, it now has a "permissive" (p. 5) function, and in some instances—such as in Poland—it may even have a decisive role. Moreover, because people felt free to express themselves in the period of the Solidarity movement and because there were several autonomous organizations doing opinion polling, the reported opinions of that period can be treated more seriously than the poll results reported before what may have been something of a political interlude or, on the other hand, a prelude to a fundamental restructuring of the system.

Mason follows the conventional wisdom in telling the story of Poland's crisis of the late 1970s and the developments of the 1980s, but he skillfully weaves in the data from opinion polls in order to highlight the currents of thought and sentiment sweeping over both the broad public and special groups. Following the world economic crisis triggered by the sudden rise in petroleum prices after 1973, the social compact linking Poland's elites and masses broke down. Job security, equality in wages, and consumer improvement had been traded off for political compliance, but, as egalitarianism eroded and consumer expectations were frustrated, political conformity declined. Poles became more materialist and demanding, social tensions and competition were increased, and at the same time Poland moved from a "subject" to a "participant" orientation in its dominant political culture. Yet channels of political communication

were blocked, and resentment of economic inequality grew, resulting in a series of strikes that eventually led to the emergence of the Solidarity movement.

According to Mason, "Solidarity's size and scope was both its strength and its weakness. The almost universal appeal of the independent union, and its huge membership, forced the regime's leadership to accommodate Solidarity and some of its demands" (p. 135). But "Solidarity tried to achieve everything at once. When Solidarity ran into obstacles, the myths began to disintegrate, and the movement to fragment. The exhilaration born of Solidarity's early successes gave way to apathy and fatigue. The regime capitalized on all of this and moved in for the kill" (p. 135). Nevertheless, during the course of the year, Poland became a "pluralist society without pluralist and representative institutions, so public opinion acted as a surrogate for those institutions. In this respect, public opinion played a stronger role in the political process than it does in most democratic societies" (p. 34). Mason shows that public opinion was far from uniform or static, but overall it became clear that socialist socialization had not been very effective and that Poles were "deeply skeptical" (p. 36) of the official ideology and of the political and economic systems deriving from it.

Why, then, did Solidarity fail in the early 1980s? In Mason's view it was not because of the Soviet threat or the rejuvenation of the moribund Polish United Workers party. Rather, Solidarity could not deliver economic well-being any more than the Communist party could, and, as opinion polls showed, economic well-being was the primary concern of the citizenry. Moreover, Solidarity had an identity problem. It "was more than a trade union, but less than a political party" (p. 240). It lost touch with its followers. Neither Solidarity nor the Communist party knew how to bargain and compromise—that was not the style in which they had been socialized—so they locked themselves into an ultimate confrontation.

Moving beyond the opinion polls, Mason asserts that after 1980 "the chain of cultural transmission" was "broken" (p. 237) and that Poland's political culture is moving farther from socialism. The dominant posture is one of apathy, anomie, alienation, hostility, and anxiety. Most observers would agree that the author's analysis is a fair characterization of the mood in Poland eight years after the declaration of martial law. Even a non-Communist government, coexisting with the Communist party and faced with an economic crisis of enormous magnitude, will find it difficult to change this mood.

Mason's judgments are sober and his presentation lucid. He does not overwhelm the reader with

the data but uses them to illuminate the larger points. He relies largely on the best of the secondary literature in telling the story, so that the main value of the book lies in his judicious presentation and analysis of the opinion poll results.

Mason believes that "the institution of Solidarity is moribund but the idea of Solidarity is alive" (p. 248). This judgment seems to have been borne out when in the late summer of 1989 Solidarity formed the first non-Communist government in Eastern Europe in forty years.

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DOMNA DONTAS. *Greece and Turkey: The Regime of the Straits, Lemnos, and Samothrace*. Athens: G. C. Eleftheroudakis. 1987. Pp. 254.

For centuries the water passageway from the Black Sea into the Aegean, known as the "Straits," has played a major role in the history of Europe, the Middle East, and Russia. Although the Straits were controlled by the Ottoman empire from the fifteenth century to the end of World War I, the right of foreign commercial and naval passage through them became an escalating issue in international politics during the eighteenth and nineteenth centuries as the power of the Ottoman empire declined, that of Russia increased, and Western European imperialistic rivalries engendered an ever-increasing French and British presence in the Eastern Mediterranean.

The Ottoman empire proved more resilient than its avid partitioners had anticipated, and it was only in the years 1919–20 that France, Britain, and Italy were able to satisfy their long-held aspirations for control of the Middle East. In the Treaty of Sèvres (1920), the territory on both sides of the Straits was demilitarized, and control of all commercial and naval shipping was placed in the hands of an international commission on which Turkey, the state that had nominal sovereignty over the territory, was not initially represented. In this volume Domna Dontas examines, from a distinctly pro-Greek position, the twists and turns of international diplomacy from the end of World War I to the signing of the Montreaux Convention in 1936, which returned effective control over the administration of the Straits to the Turkish state.

Dontas's focal point is the position of the Greek government vis-à-vis its status in Asia Minor and the Aegean, its access to and through the Straits, and its irredentist claims in Thrace and to Constantinople itself. Supported by the British, who saw in a strengthened Greece a means of extending indirect British influence in the area, the Greeks were able in the Treaty of Sèvres to estab-

lish a strong position around Smyrna (Izmir) on the Anatolian coast. This position was lost in the renegotiation of the peace treaty at Lausanne in 1923, following the military triumph of the Turkish nationalist forces under the leadership of Kemal Atatürk. The subsequent resolution, however, of the population transfer issue, which Dontas barely mentions, paved the way for a Greek-Turkish rapprochement that became stronger as both nations reacted in the early 1930s to attempts by Fascist Italy to expand its influence in the Eastern Mediterranean. This improved rapport in turn made it easier for the two nations to resolve differences and reach an accord during the Montreaux negotiations.

The Straits issue was one in which many nations played a role. Recognizing this, Dontas has attempted to place the issue of Greek-Turkish relations in regard to the Straits in the larger context of general European diplomacy. Here he is at best only moderately successful. He fails to recognize the important role played by Woodrow Wilson in 1919. All too often one must accept his assertions of impact or influence without appropriate explanation.

The virtue of this volume is that it makes available detailed information on Greece's role in international diplomacy regarding the Straits question. Its weakness is that it is too much a chronicle of who said what and who replied what. A single paragraph will often contain references to various topics whose interrelationship is unclear or unexplained. This is blow-by-blow, old-fashioned diplomatic history, dull and difficult to wade through and only occasionally providing rewards worth the effort. Less chronological detail and more attention to synthesis and analytical explanation could have provided a more valuable study.

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IU. A. LIMONOV. *Vladimiro-Suzdal'skaia Rus': Ocherki sotsial'no-politicheskoi istorii* [Vladimir-Suzdal Rus: Essays on Social and Political History]. Leningrad: Nauka for Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe otdelenie. 1987. Pp. 213. 1 r. 10 k.

Iu. A. Limonov gives us a history of Suzdalia from its entrance into recorded history to the Mongol invasion, which incorporates recent Soviet (but not non-Soviet) archaeological and historical research, including the author's study of local chronicle writing, *Letopisanie Vladimiro-Suzdal'skoi Rusi* (1967). Under Vladimir Monomakh's descendants, population growth and town formation

made Suzdalia a political power. In the 1150s, Iurii Dolgorukii moved there to make it a springboard to seize the senior throne of Kiev. His son, Andrei Bogoliubskii, repudiated Iurii's Kiev-oriented policy, and until 1172 local nobles and burghers supported his attempts to dominate both Novgorod and Kiev from Suzdalia and his brief success in doing so. Limonov's handling of Andrei's failed attempt to make his Church of the Dormition in Vladimir an independent metropolis, reflecting the elevated claims advanced on his behalf in contemporary texts, is carefully argued. But his view that Suzdal' then, and not in 1214 as generally held, became a separate bishopric is not persuasive; his contention that theological arguments in the dispute stemmed from the clash of Greek practice with local custom is probably mistaken; and his claim that the coming of metropolitans of "all Rus'" a century later to reside in Moscow represented the "material embodiment" (p. 62) of Andrei's policy is dubious. Limonov astutely sifts accounts of Andrei's murder by his servants in 1174 to reveal the machinations of his outcast brothers, princes of the "older towns" Rostov and Suzdal', certain lesser nobles, townspeople, and clerics in Vladimir (all of whom detested Andrei's autocratic ways), Riazan's princes (who resented the vassalage he had imposed on them), and Rostislavichi princes of Smolensk whom he had driven from Kiev and frozen out of Novgorod affairs. He also argues that the autonomy of Suzdalian towns was as important as princely rivalry in establishing "polycentrism" (p. 107) in the political life of Suzdalia after the death of Prince Vsevolod Iur'evich in 1212. Limonov's topical chapters are the best in the book. One argues for the rise of town assemblies or *veches* (a term not in the sources) to share power with princes, shows how the process encouraged polycentrism, and explicates their composition (all free men) on the model of his Leningrad colleague I. Ia. Froianov. Another argues the consolidation of a minor nobility, or *dvorianstvo*, as a cohesive class by 1174 under which year the word first appears in the Laurentian chronicle and suggests that by the 1260s this group was accustomed to being rewarded with estates. The last discusses Suzdalia's prominence in the wider world. Noteworthy is Limonov's argument for flax cultivation and export to Europe and Asia, although his conjecture that north Rus' monopolized the production and sale of flax, linen thread, and semi-fabricated cloth seems far-fetched.

This is a rewarding monograph, even though A. E. Presniakov, in *Obrazovanie veliko-russkogo gosudarstva* (1918), and Ellen Hurwitz, in *Prince Andrej Bogoljubskij* (1980), anticipated Limonov's outline of Suzdalian history, and Hurwitz's book

and W. Vodoff's "Un parti théocratique" (*Cahiers de civilisation médiévale*, 17 (1974): 193–215) remain indispensable regarding Andrei's church policies.

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AZADE-AYŞE RORLICH. *The Volga Tatars: A Profile in National Resilience*. (Studies of Nationalities in the USSR.) Stanford: Hoover Institution Press. 1986. Pp. xvi, 288. \$31.95.

Azade-Ayşe Rorlich's volume on the Volga Tatars is the second in a series on the nationalities of the USSR edited by Wayne Vucinich and published by the Hoover Institution. The Volga Tatars were the first non-Russian people with a state of their own to have been incorporated into the Russian empire when their lands along the middle and lower Volga River were conquered (1552–54) by Ivan the Terrible. Since then the Tatars have experienced colonial exploitation, religious bias, and constant pressure for Russification. Gradually they have become a minority in their own country. Yet they have continued tenaciously to cling to their language, religion, and culture, showing amazing ability to survive and to endure against all odds.

Rorlich's book is a feat of condensation. Because it is a pioneer work, the author tried to cover, in one hundred eighty pages, ethnography, history, religion, politics, and culture, providing information indispensable for the nonspecialist. Although she succeeds admirably in presenting a vast array of facts on the origins of the Volga Tatars, the Mongol conquest, the khanate of Kazan, the Russian rule, the Tatar revival of the nineteenth century, the Russian revolution, the emergence of national Communism, and the current state of the Tatar community, lack of space does not permit the development of any theme in depth.

Whereas part 1 ("Early History") is the briefest, adding nothing to the accounts of Sergei Soloviev, Boris Nolde, and John Pelenski, part 2 ("The Volga Tatars and the Russian State") and part 3 ("The Volga Tatars and the Soviet State") contain a great deal of new material culled from publications in Turkish, in various European languages, and, most important, in Tatar. Access to Tatar sources, in fact, spells the difference between Rorlich's book and the writings of others, including Soviet historians who seldom learn the languages of the non-Slavic peoples of the USSR.

Rorlich is full of sympathy for the Tatars. Occasionally she tends to idealize their life and culture. Thus, writing of Tatar resistance to Russification, Rorlich states that the Tatars neither

resisted assimilation blindly nor converted to Christianity. "Instead, they engaged in a continuous process of critically reassessing both their traditional values and their place in Russian society" (p. 48). It is doubtful that many Tatars, even among the literate minority, gave much thought to such matters. Resistance to Christianization, the *sine qua non* of Russification, was instinctive with them as it is with most Muslim peoples who, in general, do not easily convert to Christianity. Yet Rorlich is always fair in her judgments and realistic in her approach to such thorny problems as the survival of the Tatar language and the relations between Tatars and Russians, including the issue of intermarriage.

To dwell on minor errors of spelling, grammar, or translation may be petty, but this book contains too many lapses to pass by. One comes across locutions such as "the leading role the Tatars enjoyed . . . was because . . ." (p. 50); "Born into a family of Tatar merchant mullahs . . . Nasiri's early education followed the pattern . . ." (p. 66); "Whoever raises against the Qur'an and Shariat, raises against the government" (p. 98). The strange word "cadra" that occurs repeatedly (pp. 146, 165) does not appear in English dictionaries and seems to be the singular genitive of the Russian word *kadr* that was borrowed from the French *cadre*, the form it preserves in English. *Perepischik* is not a translator but a transcriber or copyist (p. 66), and the Arabic *burhan* is not storm but proof, which restores sense to the title of a Tatar book that turns out to be not an imaginary *The Storm of God's Clemency* but a typical *The Proof of God's Clemency*. And, when a Russian praises the Trinity, he does so not "for ages unto ages" (p. 37) but "vo veki vekov," which idiomatically translates into the familiar "for ever and ever." Much of the blame for this must be placed on the publishers who do not meet standards traditionally set for scholarly books.

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N. F. DEMIDOVA. *Sluzhilaia biurokratia v Rossii XVII v. i ee rol' v formirovanii absolutizma* [The Service Bureaucracy in Russia in the Seventeenth Century and Its Role in the Formation of Absolutism]. Moscow: Nauka for Akademiia Nauk SSSR, Institut Istorii SSR. 1987. Pp. 228. 2 r.

N. F. Demidova is the leading living Soviet authority on the seventeenth-century Muscovite *prikaznye liudi* (chancery men). These men were career bureaucrats who served the tsars as privy secretaries (*dumnye d'iaki*) and as secretaries and clerks (*d'iaki* and *pod'iachie*) in the central chanceries and

provincial governors' offices. They most represented the principle of bureaucratic rationality in a state apparatus that was strikingly centralized, bureaucratized, and interventionist by comparison with its European counterparts. Demidova's book examines the complexion, remuneration, social status, administrative activity, and ethos of this secretarial and clerical corps in order to show that the rise of Muscovite absolutism was linked to its consolidation as a civil service corps parallel to but increasingly distinct from the military hierarchy and characterized by an increasing vocationalism and dependence on monetary remuneration.

Demidova has been working on this subject for almost a quarter of a century, and her new book is the result of extensive archival research, for the most part prudently interpreted. She avoids the common temptation to exaggerate the dysfunctionality of Muscovite government on the basis of anecdotal evidence of instances of malfeasance and misfeasance, for example, and she pays considerable attention to policy shifts over the course of the century and territorial variations in practice that affected the conditions of secretarial and clerical service. Especially valuable are two thorough and detailed chapters dealing with the complexion of the professional bureaucracy and the forms of its remuneration.

She argues that continuity in the composition of the secretarial corps through the Time of Troubles and its aftermath greatly facilitated political reconstruction, which culminated in the 1630s with a significant expansion in the number of chancery clerks and in the sociopolitical power of the chancery secretaries. The bureaucratization of the central government was further enhanced in the 1640s and 1650s by measures to contain secretarial power and keep personnel expenditures down, such as restricting social eligibility for appointment, setting limits to staff size, regulating remuneration, and permitting the nobility of the Duma ranks to regain control of the chancery directorship. In the second half of the century, the secretariat did not increase in size or political influence, but there was resumed growth in the number of chancery clerks and a near doubling of expenditures on their salaries. In provincial administration, however, there were still too few clerks, and a large proportion of the provincial clerks were paid irregularly or served without treasury salaries altogether, necessitating their reliance on "feeding" revenues solicited from the local population and on payments from irregular revenues such as customs duties. Meanwhile, Moscow had not only to mobilize more tax revenues but also to enforce the new serf order, defeat the Razin rebellion, and contain the discontent among the middle service class and urban taxpayers over

their military and fiscal obligations. Therefore, beginning on a large scale in the 1670s, Moscow responded by dispatching special commissions of chancery secretaries and clerks to the provinces to conduct dragnets of fugitive peasants, carry out censuses and service reviews, and check on the activities of local officials. These ad hoc commissions were important for the further rationalization of Muscovite government, as they enhanced central chancery supervision and coordination of the less bureaucratically developed provincial governors' offices.

It is regrettable that Demidova felt obliged to repeat L. V. Cherepnin's gross mischaracterization of the Muscovite state of the first half of the seventeenth century as an "estate-representative monarchy" in transition to absolute monarchy (p. 190); indeed, Demidova fails to make any comparisons with bureaucratic development in other contemporary state systems and shows no familiarity with the literature on that subject. There are some other disappointing omissions. She promises to examine the place of the secretaries and clerks in society but says little or nothing about their patronage connections, capital accumulation, and involvement in commerce or their cultural importance as the secular intellectual elite. More could also have been said about the growth of "paperization" in administration, practices to enforce accountability, and the role of secretaries in the decision-making process by describing in detail the routine within some representative chanceries and governors' offices.

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HEINZ-DIETRICH LÖWE. *Die Lage der Bauern in Russland 1880–1905: Wirtschaftliche und soziale Veränderungen in der ländlichen Gesellschaft des Zarenreiches*. (Studien zur Wirtschafts und Sozialgeschichte, number 7.) St. Katharinen, F.R.G.: Scriptor Mercaturae. 1987. Pp. 401. DM 56.

Heinz-Dietrich Löwe's book makes a simple statement: the peasants in Russia grew significantly wealthier during the period 1861–1914, and they grew wealthier collectively, not by splitting into rich and poor levels. Moreover, their agricultural techniques underwent rapid improvement. The productivity of their farms increased at a percentage rate faster than that which obtained in Germany and France during the same period. In short, industrialization did not burden the peasants but benefited them (for example, p. 8).

Most of the book consists of statistical elaborations on this theme. They are derived chiefly from studies made by the *zemstvos*, whose figures, Löwe

says, are much more accurate than the tsarist government's and also more meaningful. Some of the work of the *zemstvos* during the period Löwe covers was more sophisticated than anything being done in contemporary Germany or France (pp. 29–30). For a variety of reasons, most Soviet and Western scholars have overlooked these studies. Löwe uses them to make general estimates of agricultural development in European Russia (excluding Congress Poland and Finland) and also to make more detailed investigations (using archives in Moscow and Leningrad) of Voronezh, Tavrid, and Moscow gubernias.

Löwe's conclusions are new. In recent years, a few scholars (for example, Paul Gregory, Arcadius Kahan, James Simms) have pointed out that peasants in Russia were raising their standard of living during the period 1861–1914, but I do not believe that anyone has come up with a scenario quite as bright as Löwe's. I do not know of anyone who has maintained that the peasants' agricultural productivity in all regions was improving at a rapid rate prior to 1905 as well as after. Indeed, Löwe stresses this improvement so insistently that the reader tends to forget the low level at which productivity (presumably) began. It is this low level that Löwe uses as a base for measuring the rate of improvement.

Löwe's statement raises big questions, and he devotes some space to answering them. Why did Russia suffer major famines? Why did the peasants act so violently in 1905–07? Why did contemporaries, including his *zemstvo* statisticians, generally believe that the peasants were sinking into poverty and agricultural stagnation? How well he has answered these questions is a subject too large to be taken up here, but his arguments deserve serious consideration.

Except for Löwe's implied assumption that economic improvement is per se beneficial and not burdensome, I am inclined to accept his general thesis. My only regret is that his reliance on statistical evidence is so one-sided. This is not to say that Löwe has been careless in his presentation, but his conclusions sound a trifle Euclidean for one who is himself engaged in tearing down statistical castles. Perhaps it is Löwe's fascination with his own hypothesis that has led him to disregard the more prosaic but useful forms of historical evidence, such as Alexander Ermolov's study *Nashi neurozhai i proizvodstvennyi vopros* (1909), wherein Löwe would have found an explanation for famine and peasant violence that in some respects resembles his own (pp. 364–66). It is a very good thing that Löwe opposes ideological presuppositions with statistical demonstrations, but it seems to me that one of the most important lessons his book teaches is to be wary of self-

assured statisticians bearing neatly wrapped bundles.

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G. I. SHCHETININA. *Studenchestvo i revoliutsionnoe dvizhenie v Rossii posledniaia chetvert' XIX v.* [Students and the Revolutionary Movement in Russia in the Last Quarter of the Nineteenth Century]. Moscow: Nauka for Akademiia Nauk SSSR, Institut Istorii SSSR. 1987. Pp. 237. 3 r.

Historians interested in the social and cultural origins of student radicalism in tsarist Russia will find in G. I. Shchetinina's slim volume scant new material or new perspectives on this topic. They will search in vain for any insight into the social dynamics of the closed, corporate "campus life" of nineteenth-century Russia and will not discover here thoughts on the problems of student marginality and revolutionary protest. Such theoretical problems may appear relevant to "bourgeois historians," some of whose works touching on Russian student radicalism Shchetinina briefly and misleadingly summarizes, but those issues are beyond her intellectual horizons. Marxist-Leninist historical canon is her authoritative answer to conceptual problems and, unfortunately for the reader, is her stylistic model. Quotations from Lenin conclude almost every chapter, a sort of definitive closure to guarantee her ideological orthodoxy.

The scope of the work might most appropriately be described as a study of student involvement in the populist and Marxist movements between the 1870s and 1890s. One introductory chapter gathers together an undigested array of quantitative data, largely on university students. The following five chapters present a very detailed account of the ups and downs of student radicalism from the years of so-called revolutionary upsurge at the end of Alexander II's reign to the "eve of the proletarian stage of the liberation movement," presumed to begin early in the reign of Nicholas II. Each chapter lists an impressive number of student circles and of progressive students, and several pages commemorate the heroic deeds of Alexander Ulianov. The author introduces brother Vladimir Ulianov (Lenin) in a few cameo appearances. These add nothing to the work of Western historians on the importance of the years immediately following Alexander's execution to Lenin's personal and ideological development.

The tone of the discussion is laudatory and pious toward all radical youth, but Marxism is greeted like a revelation midway through the

book. One can find a similar veneration toward the nineteenth-century revolutionary movement in Russian historical studies of the 1920s, which have the additional advantage of being written with some attention to literary style. I cannot recommend this work to anyone save the most zealous antiquarian scholar of Russian revolutionary history.

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TIM MCDANIEL. *Autocracy, Capitalism, and Revolution in Russia*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xi, 500. Cloth \$45.00, paper \$14.95.

The belated appearance of industrial capitalism in Russia and its implications for the maintenance of political autocracy were the focal point of much controversy in the late nineteenth and early twentieth centuries. Industrial capitalism was at the center of populist-Marxist antagonisms among the Russian intelligentsia, caused serious ruptures within the tsarist bureaucracy and between it and the landed nobility, fueled the rather pallid efforts of Russian industrialists to seek political reform, and stimulated revisions to classical Marxist assumptions by, among others, V. I. Lenin and Leon Trotsky.

Historians, both Soviet and Western, have long pondered the relationship between autocracy and capitalism in Russia. While some have emphasized that capitalism ultimately destabilized the autocracy—tearing asunder old patriarchal relations on the land, stimulating in short and sharp fashion the formation of a proletariat, and bringing workers into proximity with the revolutionary intelligentsia and the ideology of socialism—others have stressed the extent to which autocracy undermined capitalism. According to the latter perspective, private enterprise was stifled by bureaucratic tutelage and arbitrariness. The unwillingness of the regime to accommodate representative institutions militated against the articulation of capitalist relations and the consolidation of a modern industrial society. Denied legal redress for their economic exploitation and political oppression, the industrial working class, small in number but strategically located, gradually fell under the sway of Bolshevism, which offered redemption through revolution.

In this highly ambitious study, Tim McDaniel has merged these two perspectives into a single explanatory model of a revolutionary labor movement that brought about the downfall of both autocracy and capitalism in 1917. Drawing on

such widely divergent theorists as Alexis de Tocqueville, Karl Marx, Max Weber, and Trotsky, a vast array of Soviet archival sources, published memoirs of political activists and "conscious" workers, and the recent work of Western social historians, McDaniel has produced a formidable book that should be considered among the most outstanding recent studies of the revolutionary process in Russia.

McDaniel takes as his problematic "the genesis and nature of this very rare historical phenomenon, a victorious revolutionary labor movement" (p. 3). Asserting that "it was the institutional setting of the Russian labor movement, not economic crisis or war, that distinguished it from its European counterparts and explains the uniqueness of the 1917 revolution" (p. 39), he first examines this setting in comparative perspective. What distinguished Russian industrialization, he notes, was that it was undertaken by an unreconstructed autocratic regime. Autocratic capitalism thus constituted a distinct model of industrialization, one that had its own internal inconsistencies and contradictions. Essentially, its two constituent elements, capitalism and autocracy, engaged in "reciprocal subversion" (p. 43).

In the second part of the book, McDaniel examines government labor policy in chronological fashion up to 1914. This is, I believe, the most sophisticated and systematic account we have in English. It is spiced with numerous quotations from ministerial officials who thrashed about in search of a way of capping an increasingly explosive situation and industrialists who looked askance at state initiatives, or what they called "state socialism." The Zubatov experiment, which here receives extensive treatment, was the state's most audacious attempt to secure order through the organization and mobilization of workers. Its failure intensified bureaucratic infighting and led to a paralysis of reform that persisted after 1905. McDaniel summarizes the failure of tsarist labor policy in terms of "the government's recurrent assertion of autocratic protection as the ideal solution to the labor question." This, he argues, "had adverse consequences for its own authority. By failing to allow for the differentiation of a sphere of partially autonomous industrial relations, it simply took too much responsibility upon itself" (p. 152). Economic relations thus always remained highly politicized.

The third and longest section of the book analyzes the labor movement in terms of its constituent social strata (mass workers, conscious workers, and the revolutionary intelligentsia) and salient features (organization, solidarity, and radicalism). Throughout, McDaniel maintains a subtly dialectical appreciation of traditional and modern

sources of protest and the complex rhythms of militancy and quiescence. At the same time, these chapters are a bit repetitive. By the time we get to "The Circularity of Radicalism," we know that we have been there before. More seriously, the tendency to conflate working-class culture with the culture of the labor movement transforms gender, ethnicity, and skill levels into factors sully the "purity" of the categories McDaniel chooses to emphasize.

A final section considers the October revolution as the legacy of autocratic capitalism. Although acknowledging the frequent shifts of mood among workers, industrialists, and members of the political parties, McDaniel sees the vacuum of political power and class polarization as bequeaths that impelled the Bolsheviks to undertake their insurrection. There is nothing new here, but the argument that workers' radicalization was not so much a rational—or the only conceivable—response to the immediate situation that confronted them as it was a response "rooted in more general assumptions developed under the tsarist regime" (p. 326) is a useful corrective to recent studies that focus exclusively on 1917.

In sum, it is hard to imagine a more engaging and elegantly argued account of "the closest approximation in history to a proletarian revolution" (p. 407). The decision by the University of California Press to bring out a simultaneous paperback edition is to be applauded, as this book deserves a wide audience.

LEWIS H. SIEGELBAUM
Michigan State University

L. E. SHEPELEV. *Tsarizm i burzhuaizii v 1904–1914 gg.: Problemy torgovo-promyshlennoi politiki* [Tsarism and the Bourgeoisie from 1904 to 1914: Issues of Commercial and Industrial Policy]. Leningrad: Nauka for Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie. 1987. Pp. 272. 2 r. 40 k.

L. E. Shepelev, an archivist and historian affiliated with the Leningrad branch of the Institute of the History of the USSR, has written several useful studies, including an explanation of prerevolutionary ranks and titles, a book on Russian corporations, and an analysis of tsarist industrial policy from the 1850s to the early twentieth century. In this work, the first Soviet account of relations between the tsarist government and the commercial-industrial elite in the decade before 1914, his considerable talents are again evident: a thorough knowledge of the ministerial records in the Central State Historical Archive, an appreciation of the intricate processes by which the imperial bu-

reaucracy made its laws, a straightforward narrative style, and a healthy indifference to the absurd analytical categories of Soviet historical scholarship.

Shepelev presents a masterful description of the structure and function of the Ministry of Trade and Industry, which was created in October 1905. His brief portraits of the various ministers show a rare appreciation of the significance of personalities in the shaping of governmental policy. The second chapter narrates the activities of the Association of Industry and Trade (AIT), the most important business organization. The failure of the government to elaborate a coherent program of industrial development is examined in chapter 3. The book ends with an implicitly Weberian analysis of the state's consistent refusal to accommodate nascent Russian capitalism. Recalcitrance took many forms: interminable legislative discussions, subsidies to state-owned enterprises that competed with corporations, discriminatory taxation of commerce and industry, declining investments in rail and water transport, and a policy toward cartels that vacillated between mild encouragement and threats of criminal prosecution.

The author documents the policy debates with copious quotations from ministerial files and the AIT's newspaper, *Promyshlennost' i torgovlia* (Industry and Trade). Even more impressive than his attention to detail, however, is Shepelev's subtle analysis of the obstacles, both cultural and political, that hindered the growth of Russian capitalist institutions. He convincingly shows that the commercial-industrial elite, in addition to its small size and lack of an effective national organization before the creation of the AIT in 1906, faced anticapitalist prejudices in the State Duma, where gentry and peasant representation was strong. Moreover, he does not reduce the government's economic policy to a simple reflection of class interest. (Soviet historians have written much nonsense in an effort to portray the tsarist bureaucracy as a tool of the gentry, the industrialists, or both.) The AIT's main ally in the government, the Ministry of Trade and Industry, failed to convince the ministries of internal affairs, agriculture, and justice that economic development required major reforms: firm and unambiguous legal norms, adequate capital at reasonable interest rates, and massive investments in the infrastructure. The AIT exerted influence on state policy but only by virtue of its possession of accurate statistics and its argument that industrial development was essential to military power.

The shortcomings of this account reflect its strengths. Shepelev's immersion in the ministerial files inevitably tilts his emphasis toward the bureaucratic side of the debates. Although this study

is the first detailed Soviet treatment of the AIT (without reference to the work of the American historians Ruth Roosa and Carl Goldberg), it rarely mentions the many other business organizations, headquartered in provincial capitals, whose agendas did not necessarily coincide with that of the AIT.

Working within the conventions of Soviet scholarship in the Brezhnev era, Shepelev has made major contributions to historical knowledge. Let us hope that Mikhail Gorbachev's unprecedented reforms will encourage Shepelev and his colleagues to produce even bolder analyses of Russian society on the eve of the great cataclysm.

THOMAS C. OWEN
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HENRY REICHMAN. *Railwaymen and Revolution: Russia, 1905*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiii, 336. \$38.00.

Railwaymen played a crucial role in the revolution of 1905. They initiated the October general strike and participated in the ill-fated Moscow uprising in December of that year. Repressions following the uprising struck the railwaymen with particular vehemence and seriously undermined their future role. Henry Reichman has undertaken the formidable task of assembling sufficient data on a vast and varied army of railwaymen employed by thirty-three major state and privately owned railroads. It is indeed a very difficult job even to attempt to compile meaningful generalizations about such diverse groups of employees (over seven hundred fifty thousand) on different roads. Railway personnel range from high-level administrators to track maintenance crews and crossroad guards. This kind of variety makes "railwaymen" most difficult to categorize and define. The author valiantly struggles with the problem, but the task is too immense for a monograph. Reichman shows the growing radicalization of the lower strata of railwaymen during the course of 1905, but, given the turbulent and dynamic course of events during that year, and competition between political parties and unions, the picture is not always clear. In fairness it must be said that it would be impossible to accomplish the task set by the author in a single volume; there is still need to pursue the subject on a more restrictive basis, namely through histories of various railroad organizations, individual regions, and particular railroads.

Reichman's work is based on extensive research in Soviet archives and represents a high level of scholarship. The list of archival sources consulted is very impressive, and he shows what can be done by using Soviet archives. The book is well edited,

and very few errors are to be found. The only weak spot is in the index, which contains mistakes and omissions.

The strengths and the shortcomings of the book are in its bias. The author is very sympathetic to the Bolsheviks and essentially agrees with their interpretation of events. He accepts Lenin's view that the working class on its own can develop only trade-union consciousness and that political direction must be provided by the party. This assumption gave him the opportunity to follow the path of Soviet historians and, by questioning their most conspicuous claims, to produce a more credible version of Bolshevik success. Reichman's weakness stems from his reliance on Marxist conceptualizations that concentrate on class consciousness and class antagonism. His principal target is the All-Russian Railroad Union, an organization that strove to unite the varied groups of railwaymen under an umbrella organization dedicated to the achievement of common political concessions from the regime. Its principal goal was to organize a general strike for that purpose. The leadership of the union was primarily made up of liberals with ties to revolutionary parties. Social Democrats, particularly Bolsheviks, were leery of liberals becoming involved in political activities among the workers. From the start, the Bolshevik organization in Moscow embarked on a policy of undermining the influence of the railroad union. They formed their own union with an almost identical name (Union of Railroad Employees) and tried to recruit workers into it in order to gain control of the railroad union, and, when they were unable to do that, they withdrew from the union. When the railroad strike was initiated by the railwaymen in October, the Bolshevik position was ambiguous. In my opinion, the Bolsheviks opposed the October general strike led by liberals and tried to sabotage it. This, of course, is not the view of the author. He makes every effort to denigrate the actions of the liberal leadership of the railroad union and to extol the activity of the Bolsheviks.

As an example of such bias, I would point to the discussion of the attitude of the Bolshevik committee in Moscow toward support of the October general strike. Recollections of Bolshevik participants in the events admit the overcautious attitude of Bolsheviks to the general strike, but Reichman rejects them. For example, a Bolshevik participant tells us that prior to October 10 only two members of the Bolshevik Moscow committee supported a general strike, while seven opposed it. At a tumultuous Bolshevik conference of the entire Moscow region on that day, which was attended by eight hundred to one thousand workers, there was such overwhelming and enthusiastic support among rank-and-file workers for the railroad strike, al-

ready in its fifth day, that the Committee was moved to support the strike. Even then it was not until October 12 that a leaflet calling for a general strike was issued, indicating that there still remained some reluctance on the Bolsheviks' part to join the strike in progress. Reichman disregards this evidence, while claiming steadfast support of the Bolsheviks for the strike, although he distinguishes support for a railway strike from support for a general strike. He is entitled to his opinion, and one could disagree with him, but his approach is simply to disregard long-accepted opinions supported by evidence.

The founding of the railroad union has been associated with the liberals' Union of Unions. Reichman hardly mentions the Union of Unions, dismissing its influence out of hand (one entry in the index and that only in passing). Moreover, he disregards information that would support the opposite point of view. For example, literature dealing with involvement of liberals in 1905 is not discussed, and works of S. Galai, who has written extensively on this subject, are not even listed in the bibliography.

Notwithstanding the above comments, the book is a valuable addition to literature on the revolution of 1905. It nevertheless needs to be used with caution because, although it tells us much about the activities of the Bolsheviks among the railroad workers, it says very little about the role of other political groups among them.

WALTER SABLINSKY
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ZENOVIA A. SOCHOR. *Revolution and Culture: The Bogdanov-Lenin Controversy*. (Studies of the Harri-man Institute.) Ithaca: Cornell University Press. 1988. Pp. x, 258. \$24.95.

Alexander Bogdanov (1873–1928), V. I. Lenin's rival between 1904 and 1909, when he was expelled from the Bolshevik wing of the Russian Social Democratic Workers party, was also a complex and innovative thinker. He wrote on philosophy, economics, sociology, and the natural sciences (he was trained as a physician) and was a pioneer of systems theory. Yet, until recently, he was a relatively unknown figure, and no full-scale study has been done of all of the dimensions of his life and thought. He was off-limits to Soviet scholars for many years because of Lenin's hostility to him. Western scholars have focused on Bogdanov as Lenin's rival (first within the party and then outside it) and as the inspirer of *Proletkult*, a truly grass-roots movement that flourished between 1917 and 1921.

Zenovia A. Sochor focuses on Lenin's and

Bogdanov's different visions of the relationship between revolution and culture and the implications that those visions had for the development of Soviet society, especially Stalinism. She maintains that the conflict between Lenin and Bogdanov had permanent consequences, for the struggle against Bogdanov revealed and contributed to the authoritarian aspect of Leninism and created the preconditions for the political control of culture that followed. The book is divided into four parts. In part 1, "Points of Departure," Sochor sets the background for the controversy in terms of revolutionary praxis ("a revolution, by definition, must change the political culture" [p. 16]) and describes the main tenets of "Bogdanovism," treating it from a political science perspective as a model or system, so that the historian is left wondering whether there is a pre- and postrevolutionary Bogdanov and whether the three volumes of his major work, *Tektology*, published in 1913, 1917, and 1922 (the 1922 edition included all three volumes), display important shifts in emphasis and approach.

Part 2, "After October—which way to Socialism?" treats Lenin's and Bogdanov's views on the relationship of war and revolution, concentrating on their contrasting opinions on German state capitalism, war communism, and the long term effects of war. Sochor then discusses Lenin's views on the "school of capitalism," as a model of economic efficiency and labor discipline and as a work ethic, contrasting those views to Bogdanov's vision of *Proletkult* as a "school of socialism" that would not simply transmit knowledge and overcome the deficiencies of the past, for example, illiteracy, but actively nurture new ideas, values, and attitudes essential to socialism, especially collectivism and universalism (overcoming the psychological fragmentation and dependency caused, in Bogdanov's view, by professionalization, specialization, and authority-subordinate relationships). Bogdanov believed that industrial labor created a collective consciousness that would supplant bourgeois individualism but that this consciousness would not occur automatically. It had to be deliberately induced and fostered.

Part 3, "The Origins of Political Culture," contrasts Lenin's insistence on the political hegemony of the party with Bogdanov's insistence on the "cultural hegemony" of the proletariat. To Lenin, cultural revolution meant the acquisition of attitudes, knowledge, and skills necessary for restoring order, building the economy, and maintaining power; it involved emphasizing class struggle, the leading role of the party, Marxist ideology, and assimilation of past culture and was foreshadowed, Sochor maintains, in the "tutelary dictatorship" and combat model of Lenin's *What is to be*

Done (1902). By contrast, Bogdanov challenged authority and advocated equality rather than hierarchy and a reduced political realm. Antiauthoritarian and antidogmatic, he foresaw the threat to equality produced by technology itself and the problems created by psychological dependency. A Marxist revisionist, he feared that the dominance of the propertied would be replaced by the dominance of the educated and foresaw the new power relationships implicit in the separation of ownership and control. In part 4, "Laying the Foundation of the Soviet System," Sochor discusses Lenin's vulnerability to a challenge from the Left, whose adherents wanted to see their utopia realized, and argues that Lenin's decision to squelch the Left led to resentment and frustration. "Stalin tapped this resentment and frustration, unleashed its power, and directed it to his own ends" (p. 215). The result was a fusion of Leninism and Bogdanovism, authoritarianism and collectivism.

Sochor contends that Bogdanov offered a viable alternative to Leninism just as N. I. Bukharin did to Stalinism. Less inclined than Lenin to massive outbursts of will or to posit shortcuts to the future, the more scientifically minded Bogdanov stressed method and gradual transformation. His model for society was a dynamic equilibrium of freely moving parts rather than the centralized rigidity imposed by Lenin and Stalin. Unlike Bukharin, who became a moderate in the 1920s, Bogdanov, Sochor says, was a moderate even during the civil war. He lamented the flight of the intelligentsia, advocated a tolerant policy toward the peasants, and preached the creation of a new life rather than the destruction, hatred, and class struggle with which *Proletkult* is so often associated—erroneously in Sochor's opinion. Her book is a corrective to views that insist that, if only Lenin had lived, all would be well in the Soviet Union, but in making her case for Bogdanov, Sochor idealizes him, focusing on the aspects of his thought that are appealing to Westerners and downgrading, though she does admit them, offensive aspects. These include his exaggeration of the virtues of collectivism, which led many *Proletkult* writers to submerge "the 'I' in the collective 'we'" (p. 137); his "rather chilling indictment of 'deviant organisms'" (p. 198); the loss of the human element in his rationalistic utopia of organization and planning (satirized in Evgenii Zamiatin's famous novel *We*); and the antidemocratic proletarian exclusiveness that ignored the majority of the Russian population, the peasants, and could not but engender hostility to "bourgeois" specialists and intellectuals. The economic viability of Sergei Bulgakov's vision of collective creativity and the end of authority-subordinate relationships and of specialization in industry—a major issue in an improv-

erished society—is not discussed, nor is there any indication of whether or not these dovetail with Bogdanov's writings on political economy. Had Bogdanov prevailed over Lenin and actually had to implement his ideas, there surely would have been a conflict between his gradualism and his utopianism, between the Bogdanov of positivism, technology, and systems analysis and the Bogdanov of cultural liberation and *Proletkult*, and it is difficult to predict what he would actually have done. Nevertheless, the book provides a much needed survey of the ideas and approach of this major figure and contributes new material and a fresh perspective on the issue of the link between Leninism and Stalinism. It is required reading for students of Russian and Soviet history and society.

BERNICE GLATZER ROSENTHAL
Fordham University

VLADIMIR N. BROVKIN. *The Mensheviks after October: Socialist Opposition and the Rise of the Bolshevik Dictatorship*. Ithaca: Cornell University Press. 1987. Pp. xviii, 329. \$39.95.

The subtitle of this book best describes its central concern. Vladimir N. Brovkin's thesis is that, before foreign intervention and the civil war began to threaten Bolshevik survival, a dictatorial transformation had come about as a result of the Bolshevik attempt to prevent the moderate socialist parties—the Mensheviks and the Socialist Revolutionaries (SRs)—from coming to power through free elections to local soviets. Brovkin describes a sequence of events that seems to offer a clear, even simple, explanation for the Bolsheviks' increasing tendency to force their authority on the soviets. First, there were the Mensheviks' and the SRs' electoral victories in many provincial soviets during the spring of 1918. Second, there was the Bolsheviks' disbanding of many such soviets and their "legal" and forceful infringements of the right of both workers and organized parties to practice political opposition. Third, he cites the turn to armed struggle in June 1918 by "the Right Mensheviks, the SRs, the peasants, and the workers in many cities" against the Bolsheviks (p. 297). And, finally, there was the Red Terror, which began in July and was directed against all forms of dissent.

In the process of arguing his case, Brovkin adds much to our understanding of the short but crucial interregnum from October 1917 to the outbreak of civil war in the summer of 1918. He correctly emphasizes the interconnectedness of various developments—the advent of economic disaster and foreign threat, the Bolsheviks' chaotic policies in response to those dangers, and the

regime's struggle to establish its authority. Brovkin is meticulous in outlining the factional differences among the Bolsheviks, especially in regard to the issue of political pluralism. But, although it is certainly suggestive and quite tempting in its revisionist sweep, Brovkin's central thesis is not entirely convincing.

Fundamental to Brovkin's argument is the contention that the Mensheviks and the SRs posed a viable political alternative to Bolshevik rule, one that could have won out had the electoral process been allowed to run its course even within the limits of the soviets' franchise. Indeed, Brovkin's narrative shows that the failure of the Bolsheviks' variable and contradictory policies to improve economic conditions or secure peace produced among many workers a mood of disappointment and anger that expressed itself in demands for new elections to the soviets, in the formation of the independent Workers' Assemblies of *Upolnomochenny* (representatives), and in the widespread strikes of May and June 1918. But did those problems and demands indicate a shift in the political loyalties of workers (or specific sections of that aggregate social group), from "bolshevism" to "menshevism"? In a recent round-table discussion of that question, which was published in the *Slavic Review* (Summer 1985), Brovkin argued that such a shift indeed took place, and the central section of his book is dedicated to supporting that assertion. He points out not only that the Mensheviks led the Assemblies of *Upolnomochennye* but also that their slogans and demands were in the forefront of the strikes. Even though he describes his data for provincial elections as incomplete, he contends that the Mensheviks and the SRs won majorities in almost all of the newly elected soviets. The case for a Menshevik-SR victory in the elections to the workers' section of the Petrograd Soviet is particularly suspect because of a discrepancy between the figures Brovkin cites in table 4 and those on page 243.

The difficulty is not only in establishing a precise and general picture of the parties' electoral strength but also in determining the depth of workers' support for the Mensheviks and the Mensheviks' ability to act as an alternative to Bolshevik power. No doubt, as Brovkin writes, "the workers as a social group and the Mensheviks as an organized political party shared an interest in opposing the Bolsheviks' claim to speak on behalf of the workers" (p. 175). Was it not that shared interest, as well as menshevism's historic commitment to workers' self-organization, that made the Mensheviks welcome leaders for the *Upolnomochennye* movement rather than workers' conversion to the Menshevik view of the revolution? In any case, the Mensheviks remained, as

portrayed by Brovkin, unable to provide a "coherent positive program" for the movement (p. 176). That is hardly a surprising failure given either the magnitude and complexity of the problems facing any contender for political leadership or the bankruptcy of many of the Mensheviks' former economic policies, notably the state monopoly on grain and the fixing of low prices.

Brovkin delineates carefully how the Mensheviks' position toward the Bolshevik regime evolved according to circumstance and how internal differences eventually split the party. Yet not much is said about the inner contradictions inherent in the Mensheviks' political identity, so clearly reflected in the disagreements between the party's right and left wings. The right wing had always identified itself primarily with the "democratic forces" as defined by the intelligentsia; the left wing and the party's core of labor organizers had always fixed more squarely on the working class. Regrettably, Brovkin does not relate the specific issues of Menshevik debates during 1918, such as policies affecting the food supply and the makeup of a political alternative to Bolshevism, to similar discussions in 1917. Consequently, the question of continuity (in identifying permissible political allies) and discontinuity (in regard to certain economic policies) cannot be properly understood.

Brovkin's central thesis will surely remain controversial, but his use of hitherto neglected sources and his carefully pieced narrative make this book a welcome addition to the literature on a crucial and still poorly understood period of Soviet history.

ZIVA GALILI
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ALLAN K. WILDMAN. *The End of the Russian Imperial Army. Volume 2, The Road to Soviet Power and Peace.* Princeton: Princeton University Press. 1987. Pp. xv, 443. \$55.00.

The Russian imperial army has received considerable attention in the West in recent years, thanks to the work of John Keep, Bruce Menning, John Bushnell, and others. Allan K. Wildman is now established as the authority on the chaotic final year of that army. His first volume, published in 1980 under the subtitle *The Old Army and the Soldiers' Revolt, March–April 1917*, related the response of the command structure and rank-and-file soldiers to a major political and social upheaval in the third year of a difficult and costly war. The second volume continues the story through increasing radicalization of the revolution and the consequent disintegration of central authority to the takeover by the Bolsheviks in Petrograd and

their attempts to follow the mass will for peace while at the same time reasserting control over the military from the top. An extensive bibliography is appended that covers both volumes.

Wildman has meticulously combed the immense and diverse source material—protocols, memoranda, memoirs, orders, letters, newspapers—of a confused but monumental few months of Russian history. In fact, the only area that the author seems to have neglected is foreign intelligence, such as the reports prepared by the British and American liaison officers and attachés. He has, however, made ample use of émigré materials in the West, Soviet archives, and both primary and secondary publications.

Wildman's organizational approach is traditional, chronological by chapter, with geographical and topical divisions within each. The author is especially attentive to the fate of the committee structure within military units that was permitted by both early soviet and government decrees after the February–March revolution. Since the army was so large and the paper generated by these new institutions so great, much material survives for the use of the historian. Wildman, however, is quite aware of the "blank spots" that remain and cautious in his generalizations.

The result is a detailed and careful study of the collapse of the imperial army brought about by the prolonged war, an inadequate and baffled central administration, radical infiltration, and a command structure unprepared to deal with the new political realities. The depth of the study takes the reader through a maze of events and a kaleidoscope of military units. Although the maps included are certainly useful, some additional organizational charts of the army, the government, and the revolutionary organizations would have been helpful in deciphering the constant barrage of details and in delineating lines of authority (or the lack of them).

Quibbles aside, this volume, in combination with the earlier one, will be an essential reference work for any student of Russian history and anyone interested in revolution and the behavior of military units in internal crisis. Wildman convincingly demonstrates how vulnerable the army was to the new and rapidly shifting course of politics. But that vulnerability varied widely from unit to unit, depending on historical background, military activity, organizational structures, personalities, accidents, and timing. Although the army certainly was collapsing in the late summer of 1917, its real condition defies several commonly accepted notions: war-weariness was matched by a surprising ability to fight; desertion was not a major problem until the approach of the Bolshevik-engineered armistice, that is, after the October revolution;

officers and soldiers often achieved a surprising degree of cooperation and harmony in unit committees and local soviets; the northern front, closest to revolutionary Petrograd, was predictably in far worse shape than the distant and more militarily active southwest front, but even in those areas conditions and reactions varied considerably from unit to unit and from time to time.

In his conclusion, the author points the way for more study of the civil war period that followed, and, though a number of studies of that period exist, there is nothing comparable to what Wildman has produced on 1917. If anything, however, the civil war may be even more complex and difficult to decipher. Can we then look forward to a third volume?

NORMAN E. SAUL
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LYNNE VIOLA. *The Best Sons of the Fatherland: Workers in the Vanguard of Soviet Collectivization*. New York: Oxford University Press. 1987. Pp. x, 285. \$29.95.

In November 1929, the Communist Party Central Committee announced a drive to mobilize twenty-five thousand proletarian volunteers for extended service in the countryside to aid collectivized agriculture. This book is a study of the recruitment and deployment of these cadres and of their interaction with the process of collectivization. What began as a great crusade (borrowing R. W. Davies's term in *The Socialist Offensive: The Collectivization of Soviet Agriculture, 1929-30* [1980]) ended in a state of siege as these staunch partisans of Soviet power and socialism found themselves opposed by hostile peasants and rural officials. In telling this story, Lynne Viola offers glimpses both of the mentality of proletarian cadres in the midst of the First Five-Year Plan and of the chaotic, turbulent, unknowable, and uncontrollable state of the Soviet countryside.

The strength of the book lies in the source material on the volunteers' experience, particularly thousands of letters they sent to newspapers, trade unions, and factories. Many of these are from Soviet archives. The author is at her best when drawing on these rich insights into the subjective experience of proletarians in an alien world: the volunteers' attempt to impose factory methods of work discipline—starting bells, piece rates, orderly meetings—on collective farmers, their sense of heroism and sacrifice, and their feeling of betrayal and abandonment by factory workers no longer interested in their experience and suffering. There is rich material here for a study of the mentality of the stratum of the

working class from which the volunteers were drawn; they were skilled, experienced (averaging twelve years of factory work), mature (ages 23 to 39), male (92 percent), and party members (80 percent).

The execution of the book, however, does not live up to the promise of the material, the best of which remains raw and underused. A basic conceptual problem is a confused focus. Is this a book about collectivization or about the part played in it by the volunteers? At first (through chapter 4), the two "campaigns" are treated as distinct phenomena. Later they seem to be the same, but the relationship between the two is rarely made clear. The presence of seventy-five thousand other urban cadres engaged in the same task is ignored. The decision to employ the volunteers is described in a poorly substantiated chapter on the failure of the New Economic Policy (NEP), but the actual politics of the decision are absent from the discussion. Consequently, it remains unclear what the perceived tasks of these urban workers were. There is much talk of taking "proletarian consciousness" to the "spontaneous" countryside (p. 91) but little clarification of what this meant in practice. The identity of "rural officialdom," the volunteers' chief nemesis in the countryside, is similarly unspecified.

The author's voice and perspective also lead to confusion. Viola has appropriated the language of the time but whether ironically, consciously, or unconsciously is unclear. In general, the style is metaphorical and teleological: for example, "swept along with the unyielding tide of socialist construction" (p. 101) and "history moved in favor of the working class" (p. 65). The organization is undermined by excessive repetition; the book deserved better editing.

Viola concludes that the campaign of the volunteers was a "success" (pp. 213-14). But the evidence presented here suggests the opposite. The recruitment campaign was indeed a success, supporting the author's contention that urban workers supported the center's drive for domination over the countryside: seventy thousand workers volunteered for a task whose rewards were moral, not material. But, once the chosen twenty-five thousand arrived in the field, they failed to establish control over the "spontaneous" process of collectivization; they failed to comprehend rural mores; they lost the support of the center and were withdrawn within two years. The most successful volunteers were those who tried least to transform the countryside along industrial lines (p. 160). When the campaign officially ended in 1931, most had left the collective farms they were meant to lead. Many remained in the countryside, but in positions in the rural party apparatus,

perhaps becoming the type of cadre they had been sent to purge. The key question posed by the evidence—to explain the failure of the program so enthusiastically supported at its inception—remains unexamined.

Viola does not shy away from other big questions. She presents a picture of a benign central state, unselfish and heroic urban workers, corrupt local officials, and backward and tradition-bound peasants and suggests that the First Five-Year Plan was the inevitable culmination of the Russian revolution, necessary to replace the failed policies of NEP. This revolution from above had substantial support from those with the “right stuff,” the socially correct urban proletariat, who supported Stalinist goals (and Stalinist methods?). These propositions are crucial to our understanding of this period of Soviet history and demand to be tested thoroughly and rigorously, with respect for the evidence and sensitivity for all sides of the question. That these propositions are not proven here should not deter future scholars from continuing to investigate them.

DIANE P. KOENKER
University of Illinois

DANIEL THORNILEY. *The Rise and Fall of the Soviet Rural Communist Party, 1927–39*. New York: St. Martin's. 1988. Pp. xiii, 246.

In this book Daniel Thorniley explores the composition, structure, and work of the rural Communist party in the years 1927 to 1939, concentrating mainly on the regional party committee (*raikom*) and the primary party organization or cell. This well-researched study is based on a wide variety of primary sources, including major Communist party periodicals and the Smolensk Archive. Thorniley argues that the rural party was not the monolithic entity it is presumed to be in the standard totalitarian model but a chaotic organization that often proved surprisingly resistant to orders from above and incapable of implementing policies in a way that would suit the central authorities.

Thorniley begins his study with an exploration of the rural party on the eve of wholesale collectivization. Isolated, small in numbers, and composed largely of poorly qualified workers, the rural party was an ineffective instrument for change in the 1920s and was unable or unwilling to assume the coercive role that Moscow demanded of it when the grain requisitioning crisis began.

During that crisis, Moscow subjected the rural party to an administrative purge in an attempt to transform it into an instrument capable of carry-

ing out orders. The purge, however, failed to raise the qualifications of the rural party sufficiently to gain the trust of Moscow; as a consequence, the collectivization campaign of 1929–30 was carried out largely by outsiders and above the level of the cell. According to Thorniley, “Much of the regime’s approach to mass collectivization was poorly prepared and *ad hoc*” (p. 34), in large part because of the weak party administrative apparatus in the countryside and its tendency to resist or distort central policies either through inertia or through violent extremism in policy implementation. Moscow’s response to that state of affairs was to attempt to restructure the rural party, increase recruitment, and purge its ranks of recalcitrants. During the years of collectivization, the size of the rural party increased dramatically, and beginning in 1931 it began to assume a major and direct role in the administration of the agrarian economy. The rural party continued, however, to be unwieldy and frequently recalcitrant. During the famine of 1932–33, sectors of the rural party, especially in the Ukraine and the North Caucasus, resisted the requisitioning of high quotas of grain and sought to ward off famine by hiding grain, falsifying crop yields, and simply refusing to carry out orders. Moscow responded to those acts of rebellion first with a purge of regional party organizations in the Ukraine and the North Caucasus and then with a purge of the entire party apparatus. According to Thorniley, Western scholars have overlooked the important connection between the agrarian and rural party crises of 1932–33 and the general party purge of 1933.

The purges of 1933–37 had a devastating effect on the rural party, decreasing its size by 1938 to one-third of its mid-1932 membership. The party’s role in the countryside declined, and its rural administrative structure very nearly returned to that of the 1920s. Moscow’s attempt to gain control of its rural party through the traditional instrument of the purge failed and led instead to the decimation of the party in the countryside. The result, therefore, was not a monolithic party organization but a weak rural party network capable of administering the countryside only by resorting to periodic mobilizations and campaigns of repression. Repression became a substitute for control. Not until after 1938 did the size and role of the rural party begin to return to those of the collectivization years.

Thorniley’s study of the Soviet rural Communist party is a first-rate contribution to a growing body of recent Western works that have endeavored to explore the dynamics of Soviet history in the 1930s outside of the still-dominant totalitarian model. Thorniley challenges the standard totalitarian model by arguing for a less than omnipo-

tent central leadership in Moscow and a rural party that sometimes seemed to behave like a power unto itself. Like the historian J. Arch Getty (*Origins of the Great Purges: The Soviet Communist Party Reconsidered, 1933–38* [1985]), Thorniley seems to conclude that the rise and fall of the rural party was, in part, a function of state building (however capricious and ineffectual). Unlike Getty, however, he places major emphasis on the rural party's acts of political resistance. The main problem with the book is its much too narrow focus on rural party administrative bodies. The study lacks a sense of the surrounding reality—the relation between the rural party and other administrative institutions, the relation between rural Communists and peasants, and the role of the rural party in collectivization. More attention to regional party publications might have enabled Thorniley to fill in the bigger picture behind the skeletal structure of the party. Nevertheless, that problem should not detract from the high caliber of this work and its contribution to a new literature on the 1930s that carries the critique of the totalitarian model from the Russia of the New Economic Policy to Stalin's Russia, showing that the repressive policies of the times were often based less on all-embracing Orwellian controls than on administrative disarray and a pervasive political barbarism.

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REGINE ROBIN. *Le réalisme socialiste: Une Esthétique impossible*. (Aux origines de notre temps.) Paris: Payot. 1986. Pp. 347. 180 fr.

Much has been written about socialist realism—both in defense and in scorching offense. Andrei Siniavskii's concise and brilliant essay "On Socialist Realism" (1960) has been for many of us the final word on the subject. Thus, in view of today's *glasnost* and *perestroika*, who on earth wants to know more about socialist realism with its defunct *esthétique*, whether possible or impossible?

The astonishing thing is that Régine Robin's rich study of socialist realism is spellbinding. It is a work of meticulous scholarship. It is timely. In addition, Robin writes beautifully. It is to be hoped that the forthcoming English translation from Stanford University Press will do her craft and art justice. Also, let there be an index, which the original does not have.

Robin's book is timely because *glasnost* makes it mandatory for the very few Western sociologists of Soviet culture to focus as objectively as possible on the pre-*glasnost* period. Reexamination of cultural, ideological, and myth-making linkages and

breaks, of continuities and ruptures, has become especially important because the official Soviet ideological catechism is in disrepair. It had been ailing for quite some time before Gorbachev's ascent, and it presently provides only murky guidelines for understanding evolving official values and the values of the rapidly splintering counterculture. When borders between consent and dissent seem to oscillate, it helps to look back. That is said in full acknowledgment of Alexander Dalin's wise apprehension of the abuse of Russian history when it is made "to serve partisan political ends" ("The Uses and Abuses of Russian History," in Terry L. Thompson and Richard Sheldon, eds., *Soviet Society and Culture: Essays in Honor of Vera S. Dunham* [1988], p. 191). Robin's scholarship is free of that.

Robin's research design and purpose are very clear. Her book is neither a survey nor an anthology. It is an open-ended, question-posing study of the very concept of realism in Russia's nineteenth-century obsessive literary philosophy and of its key elements as they surfaced in Stalinist socialist realism and settled most ponderously in the major didactic novels of the 1930s, not without creating tensions within their own confines.

As Léon Robel has observed, Robin's work is passionate. The personal vulnerability of the young French true believer that the author once was also lingers in the book. That seems to have helped her become an astute analyst of her own inevitable disillusionment. She therefore focuses, among all of the genres, on the novel of high Stalinism and sticks to the study of orthodoxy rather than to what the French and the Russians call "literariness": "j'ai voulu m'installer pour y voir clair au coeur de l'orthodoxie et non au coeur de la littérature." And, despite years of research, this study impressively remains, as the author notes, a work in progress—"un travail encore en train de se faire, une réflexion encore à reprendre, une parole qui poursuit ses questionnements" (p. 28).

The first of the book's three parts deals with the noisy chaos of the 1934 Writers' Congress, in which Robin sees the emergence of Soviet culture. It was, indeed, an insane, cruel, and curiously spirited happening against the background of the advent of fascism. "L'Obsession réaliste du XIX siècle russe" is the title of the second part, in which realism is shown to be not style but tenacious *Weltanschauung* crashing down as a huge burden on literature. In the third part, entitled "L'Obsession de la transparence," Robin examines the canonized positive heroes of the 1930s. They were both redundant—and problematic.

Occasionally Robin disagrees with Rufus Mathewson and Katerina Clark. Being an old

hand at trying to see through the tortured Stalinist legacy, I also here and there disagree with Robin's reading. In my case, however, that would be petty and pointless. Robin's book is excellent.

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AFRICA

A. ADU BOAHEN. *African Perspectives on Colonialism*. (James S. Schouler Lectures, 1985; The Johns Hopkins Symposia in Comparative History, number 15.) Baltimore: Johns Hopkins University Press. 1987. Pp. viii, 133. \$19.50.

In this book, A. Adu Boahen attempts a synthesis of secondary works detailing African initiatives and responses to colonialism in the late nineteenth and early twentieth centuries. He also provides insight on colonialism's political, social, economic, and psychological impact on contemporary Africa.

Boahen departs from what he considers the "Eurocentric point of view" (p. vii) of historians who focus on the origins, structure, and impact of colonialism. Their emphasis, Boahen claims, is on Africa as "primitive, static, and asleep" on the eve of colonialism (p. 23). Instead, he writes that on all fronts—economic, political, social, and intellectual—the mood in Africa was one of change. Colonialism, he argues, interrupted this change.

In the economic realm, the Atlantic slave trade was replaced with commodity trade in such items as ivory, wild coffee, rubber, and palm oil. Boahen notes that this trade depended on a "gathering based economy" (p. 4) involving individuals outside the traditional merchant class or aristocracy. As a result, wealth was more evenly distributed.

On the political front, the author shows evidence that centralization of certain empires promoted greater stability in Africa. He points to such examples as the Sokoto in Nigeria, the Tukolor in Masina, and the Machemba in Tanzania. Boahen highlights Africa's experiments with constitutionalism in such kingdoms as the Grebo in Liberia and the Fante Confederation in Ghana. He examines the experiments with military modernization of Samori Ture in Guinea and Menelik II in Ethiopia. Boahen describes the impact of Islam and Christianity on African societies. He also gives attention to the small European-educated African intellectual elite who developed its version of Pan-Africanism called Ethiopianism.

Boahen overstates his thesis that Africans had become human beings as opposed to mere commodities by the 1880s. The Atlantic slave trade had ceased, but the enslavement of Africans continued to be a problem in the late nineteenth

century. He captures, however, the optimism and miscalculation of African leaders who did not realize that the Europe their ancestors had encountered in the fifteenth century had become superior technologically in the late nineteenth century. Equipped with the maxim gun and breech-loading rifles, European armies reduced African sovereign states to conquered colonies by the early 1900s with Liberia and Ethiopia retaining nominal sovereignty.

The author touches on the arguments of such theorists on colonial rule as V. I. Lenin, J. A. Hobson, R. Robinson, J. Gallagher, D. K. Fieldhouse, W. Rodney, and T. B. Kabwegyere. But their complex arguments cannot be examined fully in the book's 133 pages. Boahen's analysis is necessarily cryptic, especially when he writes, "I find these explanations both unconvincing and inadequate" (p. 29).

Boahen offers an interpretation of the colonial system from the 1880s to the 1970s, a period he considers a "watershed" (p. 111). This book differs from carefully documented and researched studies such as his *History of West Africa: The Revolutionary Years, 1815 to Independence*, coauthored with James Webster. Based on secondary works, this book might provide the foundation for more exhaustive studies that use primary materials. But he is correct in noting the absence of an African viewpoint from most studies on colonialism and in signaling the need for contemporary African leaders to take the colonial impact into account in formulating their development strategies.

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FRANÇOIS RENAULT. *Tippo Tip: Un potentat arabe en Afrique centrale au XIX^e siècle*. (Bibliothèque d'histoire d'outre-mer, new series, number 5.) Paris: Société Française d'histoire d'outre-mer. 1987. Pp. 360. 220 fr.

One hundred years ago the leaders of the major European powers met in Berlin, declaring their determination to combat slavery and staking their claims to the continent of Africa. But, as so often with summit meetings, their statements were declarations made in a vacuum; they assumed Africa was a *tabula rasa* and ignored the realities of the subject on which they pronounced. Nowhere was the rhetoric and ignorance more intense than in the vast Congo River basin, the Heart of Darkness in the perceptions of the day. Yet this region was far from being an area "untouched" by the outside world. It had been mauled, in fact, by a vast network of commercial contacts and military power stretching inland from the East African

trade entrepôt of Zanzibar to the upper reaches of the Zaire River and beyond. Uncounted tons of ivory and vast numbers of slaves were marched out of this huge area. And at the center of this loosely woven network was Hamed ben Muhammed al-Murjebi, better known as Tippo Tip.

Among a galaxy of fascinating personalities that marked the time and place—Leopold II, the Belgian king; Henry Morgan Stanley, the intrepid “explorer;” Monseigneur Lavigerie, founder of the most influential Catholic missionary order in the area; Ngongo Leteta, an African subaltern of Tippo Tip to the west of the Zaire River; Mirambo, an important African leader along the caravan route to the east; Sultan Barghash of Zanzibar; John Kirk, British consul at Zanzibar—Tippo Tip stands out as one of the most extraordinary: a consummate businessman, a brilliant diplomat, a competent administrator, and a man of lively intelligence, restless energy, and extraordinary vision. Although he spent the greater part of his life in the area between the Upper Congo and the chain of Rift Valley lakes that marks the eastern border of Zaire, his story draws on an area stretching from Cyrenaica on the Mediterranean coast to the Zambezi River and from Luanda and Banana on the west coast of Africa to Zanzibar on the east. Though not the ruler of these areas in any institutional sense, he was a powerful presence in just that area in which Leopold II sought to carve out his own state in the Congo River basin.

Tippo Tip's relations with Leopold form the implicit centerpiece to this story. It was a relationship marked with ambiguity and contradiction, in which Tippo Tip became alternately the ally and antagonist of Leopold in his various schemes. As Tippo Tip's trade network—built on exploiting ever-new areas—moved to the west, the Zaire River system became increasingly attractive as his principal export channel. At the same time Leopold sought to extend his control over the river, as the basis to extending his claims as far as the Nile and farther east still. For a moment their ambitions converged, and Tippo Tip became the “governor” of the eastern regions for Leopold's *Etat Indépendant du Congo*. But, as Leopold's schemes fell apart, as European military power in this area grew, and as Tippo Tip increasingly found himself cut off from his base by German colonization on the east coast, this alliance—always strained—fell apart. Tippo Tip became cast as the “Great Satan” in the area—as the source of the slave trade, the principal obstacle to “civilized” rule, and a Muslim to boot.

François Renault's volume is the first full-length biography of Tippo Tip by an independent historian. It is really two books in one volume. The greater part is a conventional diplomatic history,

detailing Tippo Tip's complex relations with outsiders and based on the exhaustive documentation of the various European actors in the area. The scope of this documentation, to which Renault brings a careful, critical eye, is impressive: from the secondary accounts of the multitude of European military personnel in the area and the private correspondence of Leopold II to the theses and publications of many Zairean scholars, a welcome recognition of their work. This dimension of the book is a significant contribution, providing a whole new perspective on the various schemes, maneuverings, and fitful plotting that characterized the establishment of the European colonial presence in this vast region.

The “second book” within this volume relates not to Tippo Tip's role in the international diplomatic arena but to his relations with the African peoples in the area of his influence. This story is less heroic; in fact, it forms an appalling history of callous brutality. It is a history much more difficult to elicit through written documentation, and Renault's analysis here is less sure, less successful. But there is a more serious problem here than simply the lack of locally generated written sources. We are not offered a single example of the impact of Tippo Tip's actions as one factor in the larger unfolding history of local groups. The interplay of external power and specific local actors is given little attention, and in its attitude toward Africans the intellectual approach tends to mirror the contemporary written sources. Africans are treated as one vast undifferentiated group, as the voiceless victims of the policy of more powerful actors; they are everywhere simply *les autochtones*, a term that carries a disturbing resemblance to *les indigènes* in earlier accounts (and a term that in itself raises historical questions).

Thus, there are many strengths to this work—not least its production, with true footnotes at the bottom of the page and a useful index. Yet, despite its focus on a Swahili entrepreneur from the East African coast, this study remains essentially a book on European history in Africa, a conventional, if extraordinarily rich, diplomatic history of the area as seen from Central Africa rather than from the parlors of Europe. Within that framework Renault provides an illuminating study, both a sympathetic biography and a thoughtful, informative, and well-balanced discussion of the complex and fascinating issues that distinguished this critical, if neglected, period and place.

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TEKESTE NEGASH. *Italian Colonialism in Eritrea, 1882–1941: Policies, Praxis, and Impact*. (Historiska Institutionen vid Uppsala Universitet genom Carl Göran Andrae och Rolf Torstendahl, number 148.) Uppsala, Sweden: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1987. Pp. xi, 217. 162 KR.

This doctoral thesis bears all of the distinguishing marks of its species. That is to say, some useful scholarship is presented most inelegantly. Paradigmatic to a fault—every chapter begins with its “organizing question”—the book is also irritatingly repetitive and wordy. On the other hand, the content is soundly based on research in the available records of Italian East Africa lodged chiefly in the archives of Italy’s foreign ministry and on a wide acquaintance with the published literature on Italian colonialism and generic imperialism. Yet, interestingly, this is not another study of Italian expansionism; rather, it is written from the perspective of the colony.

Eritrea, formally inaugurated in 1890, was an artificial creation “of disparate regions with different historical and ecological characteristics” (p. 182). Italian settlement there was a chimera. Conventional raw materials also proved nonexistent, although human raw material in the form of Eritrean troops served Italy well in the Libyan and Ethiopian wars of 1911 and 1935. From the Italian perspective, Eritrea’s *raison d’être* was to facilitate penetration of Ethiopia. The colony was made totally subservient to the needs and control of the metropole. The indigenous population was regarded as “infantile,” and very few of the inhabitants received a Western education and even then only at primary level. Consequently, no Eritrean bureaucratic class or Westernized intelligentsia developed, as occurred in British and French African territories, to “enter into a dialogue” with the colonial power (p. 96). In short, there was no “leeway for the emergence of colonial nationalism” in Eritrea (p. 162). Indeed, until long after World War II what little nationalist sentiment did exist was attached to the cause of Ethiopian, not Eritrean, independence.

This tale may be integrated into two broader scenarios, one drawn here explicitly, the other merely alluded to. Tekeste Negash is concerned to relate his research to theories of imperialism. As explanations of Italy’s ambition in Eritrea, he finds little merit in social imperialism and the need to use foreign adventure to overcome problems at home and even less in the Marxist-Leninist formula of an imperialist stage of capitalism. He is particularly severe on the latter school of thought because of its contention that through capitalist imperialism the colony itself was modernized. On

the contrary, in Eritrea, “Italian colonial rule emphasized the maintenance of political stability which, in turn, meant a minimum restructuring of pre-colonial social and economical structures” (p. 180). Negash prefers Joseph Schumpeter’s characterization of imperialism as objectless, or at best Social Darwinian, for this postulation serves to explain the continued underdevelopment of the Third World, its colonial association with the capitalist states notwithstanding. Overt colonial domination is replaced by commercial neocolonialism. This work is, in fact, informed by the “Dependista School of Political Economy” (p. 9).

In addition, the Eritrean colonial experience reflects on present discontents in the Horn of Africa. Nationalism in colonial territories almost everywhere began in opposition to metropolitan rule. But Eritrea, under Italian administration until 1941 and then British until 1948, voiced no “articulation of the ‘new imagined community’” (p. 163). What does this suggest about assertive contemporary Eritrean nationalism? In what ways does its postcolonial genesis make it different from the traditional nationalism of former colonies? Is Eritrea’s nationalism, for example, less deep-rooted, less genuine? Negash states that the answers must await a “scientific examination” (p. 183) of Eritrea since 1941. Quite proper, but it does leave his book with a rather anticlimatic ending.

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ASIA

STEPHEN F. TEISER. *The Ghost Festival in Medieval China*. Princeton: Princeton University Press. 1988. Pp. xvii, 275.

This book is a close textual study of the *yu-lan-p’en* festival of the seventh month of the lunar calendar. The documents used are concentrated in the T’ang, with a few earlier ones from the Six Dynasties period and a few from the Sung, Yuan, and Ming. Two key noncanonical documents, the transformation text and the lecture text, are Dunhuang manuscripts. The second of these is apparently presented here for the first time.

One merit of this work is Stephen F. Teiser’s generous presentation of translations of canonical and noncanonical texts, including sutras, commentaries, lectures, liturgies, and transformation texts and incidental prose and poetry of lay writers. Differences in style and content between canonical and popular texts are sorted out here and related to their social milieux. The language of the translations appears to be carefully controlled, and Teiser has made extensive use of Japanese

scholarship. The texts are presented in chronological order in the long third chapter, which constitutes the core of the monograph.

Teiser's main argument, which is rather discursive, is that the ghost festival was synthesized from important elements of indigenous and Indian religion and that this successful synthesis led to the festival's rapid popularization throughout T'ang society from the imperial court to the peasantry. The crux of this argument is that the Sanskrit tradition of Maudgalyayana—Mu Lien in Chinese—and his heroic quest for his mother's release from the torments of hell was easily adapted to the demands of filial piety, that is, it was serviceable to Chinese core values; that the convergence of the Taoist (and classical) *chung-yuan* and the Buddhist new year on the same day facilitated the adoption of the Buddhist rite; and that the shamanic exploits of the hero excited the admiration of Chinese popular audiences.

Chapters that relate the festival to shamanism, cosmology, and the family are especially useful. The chapter on cosmology follows the integration of Indian and indigenous chthonian cults to form the core of the popular religion of late imperial times. The chapter on the family includes a discussion of the role of the *sangha* in the ancestral rites. Teiser (using, apparently, Louis Dumont's work) persuasively argues that the renouncer, by leaving the family, ironically gained unmatched power to aid the family through service to the ancestral spirits (p. 204). One might, however, quarrel with Teiser's argument that monasticism and kinship were mutually complementary rather than hierarchical (p. 209) because he offers much evidence of the encompassment of Buddhism in China by the indigenous social order as in its service to the family and in the fact that Mu Lien sought out his father before launching his search for his mother (p. 201). In a concluding chapter, the author subtly and persuasively explores the reconciliation of the apparent contradictions between the Chinese ancestral religion and Buddhist *karma* and *samsara* (p. 221).

Teiser's book is richly informative regarding the development of the ghost festival's liturgy and tradition, and the integration of indigenous and Buddhist elements is skillfully presented. But the author has not been successful in his intention to place the festival in its social context both holistically and historically. The social whole is not analyzed, and social change hardly impinges on his account of the festival. It should also be pointed out that, in his discussion of the indigenous religious context of the festival, Teiser has ignored the rites of the official altars of the abandoned ghosts (*li t'an*) for which the *locus classicus* is the *Chi-fa* chapter of the *Li Chi* and which were

later performed at least in the Sung and Ming dynasties on the same day.

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LLOYD E. EASTMAN. *Family, Fields, and Ancestors: Constancy and Change in China's Social and Economic History, 1550–1949*. New York: Oxford University Press. 1988. Pp. x, 267. Cloth \$29.95, paper \$10.95.

Calling his work an introductory study "drawing on the insights of . . . new monographic literature" (pp. vi, viii), Lloyd E. Eastman has undertaken the formidable task of synthesizing the accumulated body of knowledge on China's society and economy from the mid-Ming dynasty to the People's Republic. He has succeeded in producing a textbook noteworthy for its direct and lucid discussion of many major historical issues—among them, the question of twentieth-century immiserization of the peasantry, the role of imperialism, and China's failure to progress beyond its sixteenth- to eighteenth-century economic expansion. Equally notable, the book's style is marked by an informality and verve certain to keep students' interest.

Eastman's interpretive descriptions of aspects of Chinese society—the family, social customs, popular religions, and the economy, that is, agricultural change and commercial and technological developments—are clarified for a Western audience by instructive comparisons with historical change in Europe and the United States. The book is filled with informative detail that only rarely, as in the discussion of banking and money (pp. 108–14), seems to overwhelm the reader with encyclopedic particulars.

Eastman recognizes the difficulty of generalizing about China, where, he says (in specific reference to marriage), "diversity was the norm" (p. 31). Generalizing is especially difficult in Chinese social history, a field still in its infancy in terms of both case studies and theoretical framework. Eastman generally succeeds in maintaining an appropriate sense of the relationship between the particular and the general, only occasionally succumbing to what seems infelicitous generalizing. In his treatment of Ming dynasty bond servants in the Lake Tai region, for example, he asserts repeatedly that the region was "atypical," but he strains to find relevance in that situation for all China. "The social and economic dynamic at work in the Lake Tai region," he writes, "also seemingly had an effect, in greater or lesser degree, elsewhere in the empire [emphasis added]" (p. 74).

The relationship between the parts and the whole in the treatment of Chinese social and economic history is crucial. In a survey like this, Eastman obviously aims to focus on the whole. But can one avoid undue distortions without giving greater specific analytical attention to the very different parts of China that compose the whole? Eastman calls G. William Skinner's recent work on the periodic marketing system and the macro-regional core-periphery model some of the most "influential" (p. 115) in the field of Chinese history. Yet Eastman's treatment of Skinner's ideas is only perfunctorily descriptive, and he makes no use of them in his subsequent interpretations. It seems clear, whatever one's opinion of Skinner's specific models, that Skinner's most elemental contribution has been to alert scholars to the necessity of analyzing systematically the geographic and regional context in which historical change occurs. Eastman shuns any systematic approach in that regard, using instead a mélange of imprecise geographical designations in his survey of economic developments. For example, he refers to "three zones within the eastern crescent" and "the southern half of the eastern crescent" (p. 139); "sections of the country in the remote interior" (p. 141); and "central," "south," and "north" China (p. 169).

Eastman himself makes the connection between levels of economic development and social and cultural change, noting also that the standard marketing community was a "culture-bearing" unit (p. 120). He does not, however, refer to or make use of a growing body of literature probing the social meaning and sociopolitical implications of differential economic development.

In his conclusion Eastman argues that it was the "minimalist" form of late imperial government that led to "administrative paralysis and economic deterioration" (p. 242). Throughout the book, Eastman often seems to don statist glasses through which he views Chinese society and economy from the traditional vantage point of the political center. Thus, for example, the managerial roles of local elites are viewed not as a result of an evolving assertiveness and responsibility among these elites but as the result of the government's conscious policy to sponsor brokerage by them. Positive provincial and local developments in the early republican period disappear in the traditional statist view that "this was an age of almost complete political breakdown" (p. 91). Political activity from the center seems to precede economic and social change. The greatest source of economic stagnation in the twentieth century, Eastman argues, was political instability; the chief reason for episodes of growing violence in late imperial

China was not economic distress but political breakdown.

Students will gain much from this readable introductory synthesis. It is an important starting point from which they can explore the implications of recent works and interpretations that, given the likely limit of space, cannot be fully treated in this book.

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CH'OE SO-JA. *Tongsŏ munhwa kyoryusa yon'gu: Myŏng-Ch'ŏng shidae sŏhak suyong* [Studies in the History of Cultural Interchange between East and West: Reception of Western Learning in the Ming-Qing Period]. Seoul: Samyongsa. 1987. Pp. 345.

Under a slightly misleading title, this book contains eight articles Ch'oe So-ja has published in South Korean journals over the last decade and a half. Her primary interest is the history of Catholic missions to China from the late sixteenth through the late eighteenth centuries, and that is the true focus of this book.

Ch'oe begins by investigating how much the Chinese knew about the West before the nineteenth century. She combs court records for references to the "Western Oceans," from the first contact with Rome in 166 to the mission of Lord Macartney in 1793. She finds that only in recent centuries have Chinese officials had more than a hazy idea of where on earth Western countries were located, much less what kind of people Westerners were, what kind of governments they lived under, or what kind of values they held.

She then explores how that cloud of ignorance began to lift. In four separate articles, Ch'oe examines what the Chinese learned of Western civilization, particularly its religion and its science and technology, from Catholic missionaries. The Jesuits, who made up the vast majority of the missionaries in China at that time, concentrated on the ruling elite of China, and so does she. The Manchu emperors K'ang-hsi (r. 1662–1722), Yung-cheng (r. 1723–35), and Ch'ien-lung (r. 1736–95) come under particular scrutiny. She concludes that the emperors and their Chinese subjects were able to separate Western religion from Western technology, rejecting Catholicism while adopting whatever European calendars, guns, and medicines they found useful.

Much of what Ch'oe says about Catholic missions in China has been said before. She offers no significant new information or startling new interpretations. Only in her final two articles does she attempt to plow new ground by comparing Chinese and Korean reactions to Catholicism and

Jesuit technology. Unfortunately, her comparison is more bibliographical than conceptual. She identifies the books missionaries wrote and the scholars in China and Korea who read them but says little about the cultural background that colored their reading or the depth or extent of Jesuit influence on specific individuals.

Throughout this book, Ch'oe blurs the distinction between those who seriously grappled with Western ideas, either scientific or religious, and those who simply dabbled in the exotica the missionaries offered. Moreover, she dismisses criticism of Catholic theology as a product of Confucian conservatism, ignoring the materials mined by Jacques Gernet and others that point to a fundamental moral and metaphysical incompatibility of Confucian and Christian world views.

Because of the abundance of names and dates in the text, accessible through a comprehensive index, and because of an appendix that lists the names, birthplaces, and relevant activities of 440 Chinese who appear in records related to Catholic missionaries and their publications, this book could serve as a valuable reference work for anyone researching the history of Catholicism in China. Readers interested in original insights, penetrating analyses, and stimulating hypotheses, however, would do better to look elsewhere.

DONALD BAKER

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R. KENT GUY. *The Emperor's Four Treasuries: Scholars and the State in the Late Ch'ien-Lung Era*. (Harvard East Asian Monographs, number 129.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1987. Pp. x, 289. \$23.00.

This book is a valuable exposition of probably the largest state-sponsored literary project in world history prior to the twentieth century, the compilation of the *Ssu-k'u ch'üan-shu* (Complete library in four treasuries) under the Ch'ien-lung emperor in the 1770s and 1780s. This undertaking has fascinated scholars of Chinese history and culture ever since. Assessment of it has been especially problematic in this century when the Chinese need to take pride in their cultural heritage has often conflicted with bitterness toward the fallen Manchu-Ch'ing dynasty. R. Kent Guy helps us get beyond longstanding Chinese interpretive tendencies that have influenced the small amount of Western-language writing on the project and its "inquisitorial" dimension. He shows that the project was not a despotic imposition on a cowering, passive, or grudgingly obedient intelligentsia; rather, it grew from a concatenation of interests

among a prideful emperor, bureaucratic careerists, and men whose primary commitment was to scholarship. The impetus came from below, as well, at a time when certain dynastic conditions and a certain scholarly spirit coalesced to the advantage of many. Guy's well-written, engaging study, soundly based in secondary and primary materials, should be read by anyone interested in relations between authoritarian states and intellectual communities.

The first chapter reviews the long history of comparable projects in China in order to clarify the distinctiveness of the *Ssu-k'u* compilation and expurgation. The second explains the orientation of Chinese scholarship in the mid-eighteenth century and, most valuably, provides a thorough study of Chu Yun and his circle of litterateurs, from whom emerged the idea to launch the project. Guy is at his best, however, in the third and fifth chapters in which he skillfully delineates the complicated courses of activity through which the two parallel sides of the *Ssu-k'u* project began, grew, and were brought to conclusion. On the one hand was the prior drive to collect, correct, recopy, and write bibliographic summaries and judgments on all of the finest extant books in the realm—a stupendous undertaking in a civilization that long had been obsessed with literature. On the other hand was the somewhat ad hoc search-and-destroy campaign, which proved uncontrollable, to expunge from the culture any writings or usages pejorative toward the Manchus or their forbears. As Guy presents it, this material is in turns mind-boggling, funny, and spine-chilling.

In the fourth chapter, Guy addresses another important matter: scholarly partisanship among the *Ssu-k'u* reviewers and the results of late eighteenth-century scholarly differences in the widely used *Annotated Catalog* of books reviewed by the *Ssu-k'u* commission. Good luck and good sense led Guy to structure this discussion around the surviving original reviews written by two important contributors, Shao Chin-han and Yao Nai, who held different scholarly allegiances and whose reviews were treated quite differently by the final editors of the *Catalog*. This chapter is the least satisfactory, however. First, the author uses the term "Han learning" synonymously with *k'ao-cheng* ("evidential research") and, thus, obscures differences between Han-text rigorists and others, as well as between eighteenth-century Han learning and seventeenth-century textual research. The relations between the legacies of Chu Hsi (1130–1200) and Wang Yang-ming (1472–1529) among eighteenth-century intellectuals and the relations between Ch'eng-Chu learning as a scholarly avocation and Ch'eng-Chu learning as a state ortho-

doxy are also not clarified; rather, they are confused.

Careful readers, moreover, will notice a number of errors in this book. For example, Guy (on page 175, among others) mistakes emperors' temple names (*miao-hao*) for their personal names (*ming*). Unsatisfying to me also are some straw issues that are set up in the introduction and conclusion to inflate the significance of this book. To argue its revisionist value, Guy characterizes the state of Chinese historical studies in ways that many will find hard to recognize. Few really hold "an image of traditional society as dominated by powerhungry despots and their machiavellian minions on the one hand, and idealistic and often eremitic Confucian scholars on the other, with little room for . . . consensus between them." Few still doubt "that creativity and disputation were possible within the framework of Confucian government." And few scholars assume that "those who served at court collaborating with Manchu rule and those who did not were in conflict, separated from each other by an unbridgeable gap of suspicion, jealousy, and mutual recrimination" (pp. 2, 3, 5).

LYNN STRUVE
Indiana University

SUSAN NAQUIN and EVELYN S. RAWSKI. *Chinese Society in the Eighteenth Century*. New Haven: Yale University Press. 1987. Pp. xiii, 270. \$25.00.

This excellent book provides an intelligent summary of our rapidly changing understanding of Chinese society in a crucial century of political stability and economic and demographic expansion. Susan Naquin and Evelyn S. Rawski are distinguished contributors to the field, energetically engaged in its multinational communication networks. They cite recent Chinese and Japanese works and American conference papers as well as a full range of Western-language articles and monographs. Especially laudable are their decisions to include substantial discussions of Qing political institutions and to give full attention to the varying patterns of China's "macro-regions." The interaction between these two approaches becomes notably fruitful in a convincing argument that around 1800 the slippage of society out of the control of its rulers was especially visible in zones of new settlement and thin government and that it was only in the nineteenth century that the devolution of power began to threaten the Qing state's control of core areas.

In a book so wide-ranging in a field as rapidly changing as this one, every specialist will find something to argue with. I found the account of the macro-region of the southeast coast generally

excellent but the discussions of foreign relations and foreign trade confusingly dispersed through the book and sometimes misleading. There is no coherent consideration of relations with the Mongols. Some mention should have been made of the differences between the Ming tribute system and its more ramified Qing successor, especially in relation to the obvious fact that the European traders at Canton were managed by institutions that had no substantial relation to the tribute system. Specialists on relations with Europeans will rejoice to see so much attention paid to the Canton trade. But is it really clear that so much weight should be given to this trade as a source of the eighteenth-century growth and commercialization of the economy?

There are some difficulties of organization and coverage. Material in chapter 4, "Social Change," would have been better integrated in chapters 1 through 3, which have a great deal to say about trends and changes in the eighteenth century, some of which becomes clear only in the light of the discussions of economic and demographic growth in chapter 4. The effects of economic and demographic growth on the capabilities of a state that allowed only slow growth in the size of its bureaucracy and none in its most important source of revenue are nicely summarized in the last chapter, but earlier references to these topics are scattered and erratic in exposition. Little is said about the sources of income of various sectors of the elite. Perhaps the authors have decided that the current state of our knowledge makes coherent summary impossible, but the whole topic is too fundamental to be passed over in silence.

This book will be very useful to social and economic historians of other areas of the early modern world who want to make some use of the Chinese case for comparative perspective. It would have been even more useful to these historians if the authors had developed more fully some of their brief comments on Sino-European comparison and had done more with Sino-Japanese comparison. Such an analysis might have helped bring into sharper focus the comparative failure of the Qing state to mobilize resources and keep urbanization and commercialization under its own coordination and supervision, a failure that seems to have been one important source of the Qing state's inability to cope with the challenges of the late nineteenth century.

JOHN E. WILLS, JR.
University of Southern California

KNUD LUNDBAEK. *T. S. Bayer (1694–1738): Pioneer Sinologist*. (Scandinavian Institute of Asian Studies,

Monograph Series, number 54.) London: Curzon. 1986. Pp. xiv, 241. \$25.00.

It is hard to tell when a given academic discipline comes into existence. For decades, perhaps centuries, various people consider a common problem, reflect on methodologies that might help solve it, correspond with like-minded colleagues, compile tentative bibliographies, duplicate each other's efforts. Then one day the parameters seem to fall into place, issues sharpen themselves, an order is first implied, then accepted. A "discipline" is born.

In this erudite and delightful book, Knud Lundbaek stakes the claim for T. S. Bayer as the founder of modern Sinology. Ironically, Bayer knew far less about China and the Chinese language than did scores of his contemporaries, especially the more learned of the Jesuit missionaries. He never went near China, and he had only a handful of often inaccurately transcribed texts to work from. And, working as he did first in Königsberg and then in St. Petersburg, he was far from the centers of scholarly work on China that were slowly forming around the Chinese libraries of Paris and Rome. Yet, by tenacity and will, Bayer carved out a niche for himself in the world of learning, and his name indeed will always feature among any list of the proto-Sinologists.

Lundbaek cogently describes Bayer's hard-working background and slow climb up the early eighteenth-century ladder in an intellectual world that was fascinated by China yet still mesmerized by the urge to find some universal principle that would explain the Chinese language and provide some simple key to it—a "clavis Sinica" that would deconstruct the grammar and provide a code to elucidate the Confucian classics. The background to this story has been much explored in recent years, especially by David Mungello in his work on the Figurists and John Witek in his study of Jean-François Fouquet. What Lundbaek does is show us how the world of Chinese explorations was narrowed by Bayer into a discipline, out of which he hoped a new and broader truth would emerge.

There was little in Bayer's own early training to give him help or encouragement. A few kind and learned men gave him advice, but mainly he generated his quest from within. In a beautiful passage in the long autobiographical preface to his ambitious work, *Museum Sinicum*, of 1730, Bayer tells us how China became his obsession: "In the year 1713, while I was staying in the country, something happened to me—all of a sudden I was overwhelmed by a desire to learn Chinese. In the period that followed I worked and thought—or rather dreamed—about how to penetrate that mysterious discipline. If only I could produce

some small thing in that field I would count myself grandson of the gods and king of kings. Like a pregnant rabbit I collected everything in my burrow, whatever I could find to make up some kind of dictionary and some introduction to the rules of the Chinese language and to Chinese literature" (p. 92). It is a wondrous summary of the kind of dreams that drive academics and that they so rarely choose to express for public scrutiny.

Bayer died young, it seems of a stroke, shortly after receiving in the mail a bitterly critical review of his life's work, written by the famous French scholar Etienne Fourmont, whom Bayer had thought was his friend. Yet his story, as told by Lundbaek, is not a sad one. Despite his shaky sources and his mistaken conclusions, Bayer's head was a rich place, and his passion for his work seems to have given him deep inner excitement. With this book, Lundbaek, who in fact is a professor of medicine in Denmark, has richly added to the literature about scholars and their environments as well as to our knowledge of Sinology as a discipline and of early eighteenth-century St. Petersburg as an emerging intellectual center.

JONATHAN D. SPENCE
Yale University

BETTY PEH-T'I WEI. *Shanghai: Crucible of Modern China*. New York: Oxford University Press. 1987. Pp. xi, 299. \$17.95.

For the reader interested in a general survey of Shanghai's history, this book is the place to begin. Drawing on Chinese and English guidebooks, memoirs, and monographic studies, Betty Peh-T'i Wei provides a narrative account of Shanghai's development as a modern commercial and industrial center. She devotes most attention to the late nineteenth and early twentieth centuries; the Republican period is treated more superficially; and the post-Liberation era is not discussed at all, save for a brief analysis of the economic reforms of the 1980s.

Challenging the idea that Shanghai began to develop only after its establishment as a treaty port in the mid-nineteenth century, Wei begins with an account of Shanghai's emergence as a center of trade during the Song dynasty and "its maturing as a community during the Ming" (p. 10). After a discussion of the Opium War and the opening of Shanghai to foreign trade, Wei devotes several chapters to a description of the evolving foreign and Chinese communities in Shanghai. She describes the transition from the initial residence of foreigners in the Chinese city to the eventual creation of British, American, and French settlements (the British and American were subse-

quently combined to form the International Settlement). The Taiping rebellion blurred the distinctions between the Chinese and foreign parts of Shanghai as thousands of Chinese refugees fled to the settlements, where they were subject to government by the foreign-controlled Municipal Council, established in 1854. Despite the amount of economic and political power exercised by foreigners in Shanghai, Wei recalls the often-forgotten fact that only twelve square miles of Shanghai were administered by foreigners, compared to the three hundred twenty square miles under Chinese administration.

Chapters 6, 7, and 8 provide descriptions of Shanghai's major districts and the various foreign and Chinese groups that populated Shanghai in the late nineteenth century. Wei recaptures the parties, races, and hunting expeditions of foreigners and describes the livelihoods of Chinese merchants, compradors, entrepreneurs, entertainers, factory workers, and prostitutes. These are lively descriptions, although they often draw on sources from the 1930s for this account allegedly of the 1880s and 1890s.

The second half of the book shifts from description to a narrative of the major historical events of the late nineteenth and early twentieth centuries that affected Shanghai. Chapter 9 describes the Self-Strengthening movement and the development of industry. Chapter 10 focuses on the importation of Western ideas, the establishment of missionary schools, and the emergence of nationalism that eventually exploded in the 1911 revolution (described in chapter 11). The following two chapters discuss Republican-period Shanghai: the intellectual and political ferment leading to the May Fourth movement, workers' strikes culminating in the May Thirtieth movement and the Guomindang coup of 1927, and the Japanese attack of 1932 and subsequent occupation of Shanghai during the early 1940s. The book concludes with an epilogue that treats the Japanese surrender, the civil war, and the assumption of power by the Chinese Communist party in 1949.

Although the book provides a useful survey of Shanghai's modern history, it sometimes suffers from a lack of focus. Wei begins with an argument that "the uniqueness of Shanghai was its predominantly Chinese character" (p. 2), yet she does not follow through on this point, and much of her evidence in fact contradicts it. The absence of footnotes, though intentional, may be frustrating for readers hoping to use this book as an introduction to primary source material on specific topics. The work is also sometimes misleading, as when Wei draws on Lao She's novel *The Rickshaw Boy*—an account of a rickshaw puller in Peking—for a description of rickshaw pullers in Shanghai,

citing no further sources that would legitimate the comparison between the two cities. Nevertheless, this volume is a much-needed update to the previous narratives of Shanghai's history and provides a lively beginning for anyone seeking an introduction to the history of and literature about Shanghai.

EMILY HONIG
Yale University

NORMAN MINERS. *Hong Kong under Imperial Rule, 1912–1941*. (East Asian Historical Monographs.) New York: Oxford University Press. 1987. Pp. vi, 330. \$35.00.

This book examines the structure and functioning of the Hong Kong government from the end of the Manchu dynasty in China in 1912 to the Japanese conquest of Hong Kong in 1941. In particular, Norman Miners focuses on the question of whether the Hong Kong governor simply relayed and enforced decisions made in London or was able to act independently when dealing with local issues.

Miners starts with a brief chronological survey of Hong Kong political history from 1911 to 1941. In the second chapter, he describes the staffing, organization, and functioning of the Colonial Office in the London government. Each of the following five chapters examines the functioning of a major component of the Hong Kong government during the years under discussion: the governor and the executive council, the legislative council, the public service, governmental finances, and the structure of the Hong Kong government. Those institutional chapters are followed by five chapters devoted to analyzing three specific issues to demonstrate how the government actually functioned, with particular emphasis on the influence of London authorities compared to that of the governor of Hong Kong. The three issues examined are the abolition of the Mui Tsai system (the traditional Chinese practice whereby a poor family might, for a money payment, turn over a young daughter to a more affluent family to serve as an unpaid domestic servant), the state regulation of prostitution, and the state's control of opium.

A concluding chapter summarizes the argument that the preceding chapters illustrate: the governor of the colony had broad discretionary power, and, even when London officials disagreed with the governor, they often bowed to the presumed superior knowledge of the official "on the spot." On a few issues, however, London was adamant, although in the long run, directly or indirectly, the governor's wishes usually prevailed. In more general terms, the instructions and guidelines issued

by the Colonial Office influenced the governor's decisions and policies by providing the framework of assumptions and priorities within which the governor worked.

These are hardly surprising conclusions, and they do not constitute the chief value of the book. The book's merit lies in the clarity of its historical discussion of the processes of the Hong Kong government and of the evolution of some of the chief issues that occupied it during the three decades under discussion. This book fills a bit of a gap in Hong Kong studies. Other writers—notably G. B. Endacott—are strong on earlier decades but touch more lightly on this period because they did not have access to the sources now available. Miners himself wrote a splendid earlier book, *The Government and Politics of Hong Kong*. It is the standard work on the subject, but it deals only with contemporary Hong Kong and, in fact, has been revised three times since its original publication in 1975 in order to keep it up to date.

In sum, this study is a work of solid scholarship, written in clear, straightforward prose with a minimum of jargon. In this book, however, as in his earlier volume, Miners relies heavily on British government documents. As a result, it is history as experienced by the British administrators; the views of the Chinese population of Hong Kong, and of the Chinese authorities in China, are inevitably given short shrift.

JAMES E. SHERIDAN
Northwestern University

CHANG JUI-TE. *P'ing-Han t'ieh-lu yü Hua-pei te ching-chi fa-chan* [The Peking-Hankow Railroad and Economic Development in North China, 1905–1937]. (Institute of Modern History, Monograph Series, number 55.) Taipei: Academia Sinica. 1987. Pp. 176.

Chang Jui-te's study of the Peking-Hankow railway in the years 1905–37 focuses on two important issues: how well the railway was managed and how the railway affected the pattern of economic development in north China. A brief introductory chapter surveys the classical debate over the role of railways in economic modernization and discusses the contribution that the new economic history (the work of Robert Fogel, Albert Fishlow, among others) has made to this debate. The second chapter deals with the Peking-Hankow's financial problems, which, as Chang rightly points out, were a result of both internal management decisions and external disruptions (warlord depredations and natural disasters). The next three chapters address various aspects of the Peking-Hankow's impact on the economy of north China,

including commerce, agriculture, and mining. A brief final chapter summarizes some key points.

This monograph is based on a remarkably comprehensive survey of primary sources and secondary works in Chinese and English, with some use of Japanese sources as well. Scholars who work on the economic history of China's railways in the future will undoubtedly want to use Chang's bibliography as a starting point.

Unfortunately, the overall usefulness of Chang's painstaking work has been seriously diluted by a cruel time warp. Although published in 1987, the book was initially drafted seven years earlier. During the interval, several major monographs on closely related subjects have appeared. Even a partial listing of these competing titles would include the following: Philip C. C. Huang's *The Peasant Economy and Social Change in North China* (1985), my *The Dragon and the Iron Horse: The Economics of Railroads in China, 1876–1937* (1984), Ernest P. Liang's *China: Railways and Agricultural Development, 1875–1935* (1982), and Tim Wright's *Coal Mining in China's Economy and Society, 1895–1937* (1984). Chang is fully aware of these later monographs and indeed incorporates some discussion of them into his own text. But the result is not entirely satisfactory, and scholars working in the field will almost certainly want to consult the original monographs rather than rely on Chang's brief, derivative comments. Of course, for Western historians, these competing works have the additional advantage of being published in English, whereas Chang's work is available only in Chinese (and a very traditional Chinese at that).

RALPH W. HUENEMANN
University of Victoria,
Canada

H. D. HAROOTUNIAN. *Things Seen and Unseen: Discourse and Ideology in Tokugawa Nativism*. Chicago: University of Chicago Press. 1988. Pp. xii, 494. Cloth \$40.00, paper \$14.95.

This book is a highly sophisticated, meticulous analysis of *kokugakusha* (nativist) thinking in Tokugawa Japan. H. D. Harootunian states that "it is not my purpose to write . . . a historical narrative of the development of nativism . . . nor will it be my intention to satisfy conventional claims to coverage and account for its content comprehensively." His study, he explains, is "discourse, not history. . . . Discourse belongs to the order of language usage, the possible forms of utterances, and the particular way nativists were able to talk, write and think" (p. 3). The *kokugakusha* are a particularly appropriate subject

of study in this respect because of their uncommon interest in sounds and words.

The author is highly critical of adherents of historical common sense and their insistence on lucidity (the "tyranny of lucidity" [p. 6]). In repudiation of "the liberal-humanist appeal to open-mindedness and pluralism which authorize common sense," he opts for the use of linguistic innovation, because "resistance to familiar values and presupposition in discourse must not reproduce familiar values and presuppositions . . . by being easily readable and understandable" (p. 10). This approach places a heavy burden on readers who, like this reviewer, are inclined toward historical common sense. On the other hand, the intense cerebral concentration demanded by the author is richly rewarded because he raises the level of the study of Japanese intellectual discourse (dare I say history?) several notches above conventional studies.

The difficulties of the later Tokugawa years, the nativists concluded, resulted from the loss of wholeness in the society. For, in the opinion of a leading nativist, Hirata Atsutane, all things past and present were held together by a seamless "web of interconnecting relationships by virtue of their common descent from the creation deities" (p. 28).

This suppression of wholeness, the nativists argued, was caused by the disjunction between language and experience, a consequence of the adoption of Chinese scripts and the Chinese "mind." Thus, it was necessary to search out the original, natural Japanese language in order to restore wholeness. To do so, pre-Chinese, native literary works produced in the native idiom had to be studied.

Thus, the early nativists turned to native poetry and songs as a way to recover the fresh, authentic Japanese language. The preeminent proponent of the superiority of the Japanese language and the importance of poetry was Motoori Norinaga. Motoori, in analyzing the Japanese language, pointed to the verbal system that could establish graduated meanings and signify profound sensitivity to things (*mono no aware*). Ancient poetry composed in pure Japanese language provided the means to comprehend the pure, ancient spirit. Poetry privileged feeling over reason and brought the human and the divine together.

One of the salient points that emerges from Harootunian's interrogation of the texts, and a significant contribution to the hermeneutics of *kokugaku*, is the linkage between the nativists' perception of the Ancient Way and agricultural work and their effort to confer on the ordinary folk a sense of dignity of work. Hirata and his followers sought to give voice to the ordinary people by

establishing a condition for discourse in the knowledge, the lived experience, of the common people in the areas of work and worship. Hirata and his epigones valorized work and worship and renounced the "Chinese mind" with its artifice and contrivance. Learning, Hirata argued, should concern ordinary life.

Hirata sought to deal with the unhappiness and fear of death that he saw pervasive in the society. He envisioned a cosmological scheme in which visible and invisible things were divided. Divine intervention had fixed the invisible world as the source of the phenomenal world of visible things. Judgment concerning behavior in the phenomenal world was based on morally informed activity, which came to mean work and productive labor, the core of ethical conduct. Work was placed above learning in contrast to the Neo-confucian moral view that placed physical work below mental activity.

The creation deities created the land, and the people continued the act of creation by working to make the land habitable. Thus, the past and present, and work and worship, were linked. The business of daily life such as eating and working were religious acts, Hirata held. Speaking the Japanese language was worship; work was repaying the gods.

Hirata, like other nativists, stressed the importance of the spoken language, which remained uncontaminated from Chinese script and thought. Hence, he based his writing on speech and used short, crisp, and economical sentences. He rejected florid prose and literary embellishments because truth is disclosed in the plain, translucent language of daily speech. The author's discussion of this aspect of Hirata's thinking strikes one as being somewhat ironic in view of his rejection of the "tyranny of lucidity."

Hirata's focus on the ordinary people and daily work influenced members of the upper-class peasantry among his followers. Harootunian sees a conjunction of the original content of *kokugaku* and the upper-class peasants who sought to resolve the problem of growing social discord by returning to the Ancient Way. The village—the place of daily life, work, and worship—came to be seen as an autonomous entity removed from the center of power.

The author meticulously examines the texts of the leading disciples of Hirata, such as Ōkuni Takamasa, who linked divine creation and human creativity, which consisted of farm work. Some nativist scholars joined the imperial forces at the Meiji Restoration believing that imperial restoration would mean the return of authority to the rural community, in effect secession from the center. The Restoration, however, did not result

in a return to an autonomous communitarian order, but a new power center was established. Neither did the new order establish a truly translucent language. The nativists' vision, the author concludes, was too utopian.

My quibbles about this impressive discourse stem not from the author's shortcomings but from my own inability to deal readily with the abstract theories of numerous schools of historical and literary criticisms that the author interjects throughout the text and that obstruct the flow of the discussion. Harootunian, however, gives ample warning to the reader at the outset about his thoughts on lucidity and the importance of theory and defines his purpose and approach explicitly. So the reader has no ground to complain or hope for more cross references and comparisons with the dominant mode of thinking of the Tokugawa era or to wish for a greater effort to place the ideas of the *kokugakusha* in social and historical context.

Harootunian is modest about this monumental study. He states that he "does not claim any privileged authority for his reading of the text because it is mediated by our own situation" (p. 22). This explains his generosity in acknowledging the merits of the works of scholars with whom he differs in analysis.

The author's work reveals extensive research in the vast body of *kokugaku* texts as well as in Japanese studies of the nativists. At the same time he displays an impressive acquaintance with many Western theorists and scholars—structuralists, deconstructionists, and others. The extensive footnotes reveal the scope of his scholarship and research. His ability to interrogate arcane *kokugaku* texts is truly impressive. His "reading" of the *kokugaku* texts reveals many perceptive insights and provocative observations that should arouse common-sense historians to examine *kokugaku* and other aspects of Tokugawa intellectual developments with a more critical eye and from a fresh perspective.

MIKISO HANE
Knox College

THEODORE FRIEND. *The Blue-Eyed Enemy: Japan against the West in Java and Luzon, 1942–1945*. Princeton: Princeton University Press. 1988. Pp. xx, 325. \$45.00.

Written in vigorous, sometimes breezy prose, this bold work recounts Japanese rule and subsequent national liberation in Java and Luzon from 1942 to 1945. Theodore Friend posits that "the cross-weaving of the narrative" will "make events narrated before look fresh" (p. xvi). For that task memoirs by a few key Japanese officials and inter-

views conducted since the 1950s with some Filipino and Indonesian participants yield especially vivid vignettes of the least understood of modern empires.

Despite its subtitle this extensively documented study mainly compares Philippine and Javanese responses to wartime occupation by Japan. The author brings out the extreme contrast between the two cases: prewar Dutch dominion over the East Indies versus relatively great Filipinization under the Americans in Luzon; modest resistance to Japanese authority in Java from 1942 to 1945 versus notable noncooperation in the Philippines. Indeed, the Philippine leader Manuel Quezon tutored prewar Javanese nationalists in the Sisyphean task of gaining independence: "Hold your meetings in the open, and in front of the Dutch themselves . . . make a hell of a lot of noise! And if you do that long enough, you'll eventually get what you want" (p. 53). The author nimbly sketches the disappointment of the Javanese nationalists at not receiving independence in 1943 when the Japanese granted it to Burma and the Philippines. (Sukarno wept for joy when Japan belatedly set Java free in August 1945.)

The most effective chapters treat local assistance or resistance to the Japanese war effort and the callousness of the occupying Kenpei (military police) toward local sensibilities. Friend's irrepressibly anecdotal style deftly illustrates Filipino and Javanese resentments at face slapping by the Kenpei (although one may doubt the author's assertion that people in Japan viewed that indignity more mildly). Somewhat less instructive is the account of Japan's imperial ideology and wartime goals. Friend accepts at face value the propagandistic rhetoric of "holy war" proclaimed by Tokyo. Especially flat is his treatment of Pan-Asianism—he ingeniously labels one figure a "Shintoesque Confucian" (p. 89). The author headlines but does not fully develop the concept of Japan as a successor empire to American and Dutch authority in Southeast Asia. He relates some marvelous incidents involving individuals yet gives little more than a monochromatic portrait of the institutions of rule. Even less distinct are the economic contours of empire and the places of Java and Luzon therein.

Because the author alternates dramatic (often biographical) examples with long passages of narrative synopsis, it is sometimes hard to discern clear threads of interpretation. The eleven descriptive chapters contain surprisingly little analysis. The conclusion leans heavily on psychological, even ethological, theories of uncertain relevance in an attempt to explain Japan's wartime behavior abroad and Southeast Asian responses to military rule and to subsequent independence.

This lively book draws on and complements but does not supplant major studies by such scholars as Teodoro A. Agoncillo, Benedict R. O'G. Anderson, Harry J. Benda, Bernhard Dahm, Grant Goodman, A. V. H. Hartendorp, Joyce C. Lebra, Alfred W. McCoy, and David J. Steinberg. Poised on the boundary between political chronicle and intercultural history, this episodic treatment offers brief glimpses rather than sustained insight into a critically important topic. The volume is burdened with many inconsistencies in transliteration of foreign-language terms.

THOMAS R. H. HAVENS
Connecticut College

B. B. MISRA. *Government and Bureaucracy in India, 1947–1976*. Delhi: Oxford University Press. 1986. Pp. xv, 416. \$27.50.

The Indian Administration Service (IAS) was created to succeed the Indian Civil Service as the administrative pool providing bureaucrats to the postcolonial central and state governments. The IAS was to have served the purposes of national integration and democratization by executing the decisions of elected representatives while remaining independent of political parties and their partisan interests. In B. B. Misra's account the IAS has not met those objectives.

Misra's view of the postindependence experience is rooted in his perception of a fundamental and static opposition between Hindu social organization and ideology and the Weberian model on which the British and their Indian successors attempted to design bureaucracy. He sees the Weberian model and the civilization that produced it as based on "the rule of law" and contracts, whereas popular mentalities in Hindu society are caste based and "status-bound" and do not respect the rule of law, believe in promotion according to performance, or accept the goal of noncommunal bureaucratic impartiality (pp. 273, 371). Even the nationalist movement, whose leaders created the IAS, was committed not so much to a progressive, egalitarian reconstruction of Indian society, Misra writes, as to consolidation of elite privileges, including their own government employment (pp. iii, 272–73, and 325–26). In the absence of village-level enthusiasm for governmental decentralization and of popular protests against bureaucratic corruption, the Delhi-based reform commissions, supervisory agencies, and idealistic politicians such as Jawaharlal Nehru and Vallabhbhai Patel failed to narrow the gap between the bureaucracy and the people, between the traditions of bureaucratic privilege and authoritarianism and the goals of democratization.

On the contrary, since Nehru's death, "Sovietism," "which gives primacy to the party executive outside the legislature" and to "the exercise of power without responsibility," has grown (p. 331).

Misra focuses on personnel policy—on recruitment, training, assignment, evaluation, and emoluments of the IAS. That focus reflects his reliance on governmental archival sources that trace the periodic efforts to rectify the inefficiencies, corruption, and dilution of quality in the vast Indian bureaucracy. He acknowledges that much of the perceived decline in the quality of civil service recruits and administrative performance resulted from the expansion of government functions and the introduction of parliamentary democracy and the party system after independence. The withdrawal of British civil servants, the integration of the princely states in 1950, the reorganization of state boundaries in 1956, and the implementation of rural development plans required an unexpected increase in IAS recruitment of over 400 percent between 1947 and 1967. More than half of the new recruits entered the IAS without taking civil service examinations because politicians wanted to exercise patronage or fill newly created positions rapidly. Elected ministers at the state level were particularly eager to enlarge their control over bureaucrats working in state governments. Not only did IAS recruits' educational attainments decline in the process of haphazard expansion, so did civil servants' probity and independence from political interference.

This study will disappoint people looking for a detailed assessment of administrative performance and the relations between bureaucrats and politicians. Strong, generally negative opinions are offered about both subjects but without substantial analysis or the understanding that new solidarities and identities are evolving. Misra implies that economic development efforts have generally failed and that religious exclusiveness and preoccupation with status and caste are major parts of the explanation of the bureaucracy's shortcomings. He neglects a large body of literature that suggests more complex and nuanced conclusions.

JOHN R. MCLANE
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MARY TAYLOR HUBER. *The Bishops' Progress: A Historical Ethnography of Catholic Missionary Experience on the Sepik Frontier*. (Smithsonian Series in Ethnographic Inquiry.) Washington, D.C.: Smithsonian Institution. 1988. Pp. xii, 264. \$29.95.

Missionaries on any frontier face a dilemma of crucial significance to them and the work they do. The necessity of establishing for themselves a

comfortable living and working environment means that much of their energies must be devoted to securing supplies, growing food staples, building houses, churches, schools, and hospitals, and so on. Time given to these tasks is time away from their "true" work, that of converting locals to Christianity. Mary Taylor Huber has written an interesting anthropological account of German missionaries, members of the Society of the Divine Word (SVD), on the Sepik frontier in New Guinea from the closing years of the nineteenth century to the present and has examined how they sought to resolve this dilemma.

For Europeans the class differentiations among government employees, traders, and missionaries became blurred in remote areas where survival was the first concern of everyone. And even within their order the members of the SVD found it impossible to maintain the clear differences between fathers and brothers that had been an integral part of their lives in Europe. The need of the ordained members of the order to assist in development work took time away from evangelistic endeavors. How they reacted to their situation was further shaped by the view the Catholic hierarchy took of development work, within the context of missions, before and after Vatican II.

Huber's work is a study of the dynamics of a mission community in an extremely remote location and how it developed a Catholic church unique to New Guinea but still tied to Europe. Her book covers such diverse issues as the relations the missionaries had with the German government in the years following the Kulturkampf, the difficulties the missionaries had with the Australian government following World War I, the cultural and linguistic problems of trying to convert the diverse villagers within the Sepik region, struggles with the SVD and the Catholic hierarchies, and the relations between the SVD and the cargo cults. In the period following World War II, the Sepik mission acquired a new bishop, Leo Arkfeld, who was trained as a pilot and arrived with an airplane that he flew on pastoral visits. Very few of the mission stations had areas suitable for landing an airplane, but lack of an airfield did not stop the flying bishop. If the image of a poor missionary flying to visit outlying stations did not fit the picture homefolk had of mission work, the bishop's response was that an airplane was the best means of travel where roads were few and distances great. The bishop and his plane soon became part of the lore of the mission.

Huber's opening statement that anthropologists who study missionaries are likely to be misunderstood certainly will be echoed by historians studying missionaries. Yet missionaries did (and do) exist, and they had (and have) an impact on the

nations to which they are sent. Huber's study is not history nor is it intended to be, but, as an anthropological study, it will serve as useful reading for anyone interested in the history of missions in the South Pacific or elsewhere.

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UNITED STATES

NANCY F. COTT. *The Grounding of Modern Feminism*. New Haven: Yale University Press. 1987. Pp. xiii, 372. \$29.95.

Why did the first wave of feminism collapse after suffrage? Nancy F. Cott at last provides a satisfying answer. Cott argues that the "functional ambiguity" (p. 20) in the nineteenth-century woman's movement and the feminism of the 1910s—"its simultaneous affirmation of women's human rights and women's unique needs and differences" (p. 49)—became a painful double bind resolved at the expense of women's common bonds. Such a succinct summary testifies to the clarity of Cott's thinking, but no brief treatment can adequately convey its richness, elegance, and intelligence.

Cott begins with the nineteenth-century woman movement's "perception that the gender hierarchy of male dominance and female submission was not natural but arbitrary," a unifying thread that connected women otherwise divided on goals and tactics (p. 18). By the turn of the century, feminism had begun to emerge from the convergence of women's higher education, their increased participation in the labor force, and an atmosphere of reform. Feminism challenged the prevailing order more comprehensively than the woman movement had done, cutting loose from traditional Christianity and respectability to demand heterosexual fulfillment and economic independence, finding expression in the labor, suffrage, birth control, and socialist movements—and inevitably appealing to a smaller proportion of women.

After World War I and the achievement of suffrage, the creative tensions within feminism—between political and economic campaigns, between gender solidarity and individual opportunity, between personal fulfillment and social service—split into warring tendencies. Feminism also became a prime target of reactionaries. Because male politicians scorned woman-oriented activism and women themselves split over issues, women avoided partisan politics and continued to express themselves politically in the presuffrage modes of voluntarist or pressure-group politics. The 1920s and 1930s, Cott argues, saw women's greatest burst of associational activity.

If the form of women's activism remained much the same, the content created acrimonious debate. The National Woman's Party (NWP) appropriated the term "feminism" but divested it of the broad spectrum of social and economic issues that concerned women. The NWP placed itself in the contradictory position of advocating the equal rights amendment (ERA) while urging women "to be 'loyal to women as women'" (p. 76). The elitist, top-down campaign for the ERA collided with campaigns for women's protective legislation by the Women's Trade Union League, the Women's Bureau, and their allies. The duality of earlier feminism was shattered, its two halves placed in opposition to each other.

Cott dissects the split between these two camps in a fresh and original way, acknowledging the roles of personality conflicts and old enmities from the suffrage campaigns but emphasizing serious political and philosophical differences. The NWP, she argues, had a vision of a society of "atomistic individuals" joined only "by competition and contract"; protective-legislation advocates saw a world in which the sexes' complementary roles were "organically linked and justified by religion and tradition" (p. 141). Advocates of protective legislation failed to challenge the sex-based divisions of labor, while the NWP naively expected economic independence to follow from legal equality.

Feminism's vision of personal life also fared badly in the postwar decades. Heterosexual fulfillment, once a goal of personal liberation, became a prop of companionate marriage; the notion of a marriage based in friendship simultaneously devalued women's relationships with one another and branded women's activism the product of sexual maladjustment. The world of consumption coopted feminism, translating "the feminist proposal that women take control over their own lives into the consumerist notion of choice" (p. 172). Debates about the relation between family and economy split the relatively well-off advocates of a career-marriage combination, grounded in liberal-individualist notions of choice and self-fulfillment, from the supporters of protective legislation, who saw women's paid work as the product of family need. In yet another way, feminist unity and women's solidarity suffered.

The devaluation of gender solidarity required women to placate men in order to accomplish even minimal changes. Women in party politics had to prove their loyalty to the party at the expense of women's demands; women in unions had to fit themselves into a male-defined framework; women intent on combining career and marriage hastened to assure men that they worked for fulfillment and not to supplement men's inadequate support. The dilemma was perhaps cruelest

for women in the professions, where the reigning ideology of professional objectivity encouraged women to identify with the male professionals who perpetuated discrimination against them and to dissociate themselves from feminism. Woman professionals could thus blame failure only on themselves.

The ultimate paradox of women's lives during the 1920s is that many women found greater freedom as individuals while the specter of organized women provoked mounting condemnation. In part, this was a function of the era's reactionary spirit; in part, it grew out of feminism's own uncertainty about the relationships between gender solidarity and the rich variety of women's lives and options, between equality and difference. Cott's study is primarily a history of consciousness—of "women's willingness or reluctance to say *we*"—based on the experiences of female movers and shakers (p. 9). Cott has opened the way to the logical but difficult next steps—study of the relationship of feminism to the daily lives of ordinary women and of the relationship between women's consciousness and action.

SUSAN PORTER BENSON
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JEANNE BOYDSTON *et al.* *The Limits of Sisterhood: The Beecher Sisters on Women's Rights and Woman's Sphere.* (Gender and American Culture.) Chapel Hill: University of North Carolina Press. 1988. Pp. xxiv, 369. Cloth \$32.00, paper \$12.95.

This fascinating collection tells the story of three sisters whose lives spanned the nineteenth century. Catharine Beecher, Harriet Beecher Stowe, and Isabella Beecher Hooker were, of course, no ordinary sisters. As members of the Beecher family, they inherited a tradition of public service and a claim on the public's attention. As women, however, they lacked access to the pulpit that their father and brothers readily commanded, and so each sought an alternative forum for her ideas and activities. Catharine, who was born in 1800, became an advocate of women's education and domesticity; Harriet, born in 1811, found her niche as a writer whose work was strongly informed by the tension she experienced between domesticity and vocation; and Isabella, born in 1822, became a leader in the women's rights movement. Each confronted the problem of how women should wield power in a society that defined power and public activity as masculine prerogatives. Each struggled with that problem in ways that both reflected and shaped the nineteenth-century debate over "woman's sphere."

In this study, Jeanne Boydston, Mary Kelley, and Anne Margolis combine compelling analyses of the Beecher sisters' lives with carefully selected documents illustrating their ideas. Crisply written introductions to the women's words provide the framework for each of the book's four sections and establish context for the reader. "Shaping Experience" delineates the sisters' formative years, covering Catharine's struggles with her father and with his Calvinist faith and her turn to an educational vocation, Harriet's literary emergence and the intertwined nature of her domestic and literary lives, and Isabella's search for a satisfying vocation. "The Power of Womanhood" explores their understanding of female "nature" and their convictions about women's proper social roles. "The Politics of Sisterhood" examines their conflicting views on women's rights, women's sphere, and women's suffrage and illustrates the complex search for a female political voice in an increasingly democratic age. The final section, "Conversations among Ourselves," provides excerpts from family letters (including those of the fourth Beecher sister, Mary Beecher Perkins) and offers insights into the sisters' relationships with each other.

This brief list of topics can scarcely do justice to a book that touches on so many key historical issues. On one level, the book ushers readers into the Beechers' homes and lets us see the hearths at which they forged their lives: Catharine devising a new social role out of the tragedy of her fiancé's death and formulating a moral philosophy out of struggles with her father's religion; Harriet transforming the unbearable loss of a beloved child into a central theme in *Uncle Tom's Cabin*; and Isabella projecting her own experience with a happy and (relatively) egalitarian marriage into a vision of equal political rights for all women. On another level, the book enables readers to observe the sisters' involvement in, among other events, the abolitionist struggle, the split in the women's rights movement, the Beecher-Tilton scandal, and the moral reform crusade. On yet a third level, the book offers a running commentary on the theme of female solidarity, or sisterhood. Whereas recent historians have stressed the conditions that bound women together in the nineteenth century and separated them from men, Boydston, Kelley, and Margolis take a different tack, reminding us that race, class, and ethnicity have divided women as often as gender has united them and that even women as similar as the Beechers found that sisterhood had its limits. In this context, the authors indirectly address divisions within contemporary feminism, using their historical perspective to warn against easy assumptions of gender-based solidarity.

Historians will find this book to be a rich, varied, and thought-provoking work, one that is very adaptable to classroom use. Although offering no easy answers to the questions it raises, the book brings fresh insights to our understanding both of the Beechers and of the causes they championed.

ANNE M. BOYLAN
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R. DOUGLAS HURT. *Indian Agriculture in America: Prehistory to the Present*. Lawrence: University Press of Kansas. 1987. Pp. xiii, 290. \$29.95.

This study offers fresh and vital insights into the life and culture of the American Indian. It develops an essential understanding of American Indian culture that has not been assessed through the more conventional accounts of battles, leaders, cowboys, and defeats.

Pre-Columbian Indian agriculture achieved a remarkable level of success measured both by Indian cultural achievements and by contemporary European standards. By 1000 B.C., Mesoamerican peoples were full-time farmers who supported large-scale permanent cultures and civilizations. Indian agriculturists practiced selection, irrigation, and terracing. North American agriculture derived largely from the Mesoamerican and concentrated on the cultivation of corn, beans, and squash. About 800 A.D., Indian agriculture began to change from the diverse garden plot system to a more extensive field system. Indians in New Mexico introduced cotton and cotton textiles about 700 A.D.; the Hohokam in the Southwest became proficient in canal building and irrigation; and the eastern tribes, such as the Appalachees and Tunicas of Florida, lived in permanent settlements, planted two crops annually, and rotated their fields. Rotation, rather than fertilization, was common to Indian agriculture.

Contact with the Spanish dramatically altered Indian agriculture. The Spanish introduced wool, wheat, chili peppers, hogs (which the Indians refused), and horses. Indian agriculture in the Southwest declined as horses became more important. Despite their agricultural achievements, most North American Indians were not primarily agriculturists at the time of contact with Anglo-American societies. Indians did not accept or understand the concept of private property, of absolute land ownership and sale. Land was used but not owned. Moreover, in Indian society the women were the agriculturists, not the men, whereas American policy required that the men become the farmers, and the Anglo-European concept of tribes and tribal sovereignty was inappropriate.

Much of the history of Indian agriculture (and

Indians) since the advent of the European has concerned the alienation of Indian lands and the demise of a historically successful Indian agriculture. American Indian policy has wavered between the diverse objectives of assimilation and acculturation, on the one hand, and cultural integrity and tribal sovereignty, on the other. Both goals have been fraught with failure and misdirection. Nineteenth-century American policy was generally to make farmers out of Indians. Some groups, in fact, such as the Creek and Cherokee, successfully made the conversion by the 1830s; others, predominantly in the Southwest and on the northern plains, were enjoined to be successful farmers on the least fertile lands, without capital and educational preparation, and at a time when subsistence family farming had ceased to be viable under the best of conditions. Thus, the actual effects of Indian policy (as exemplified by the Dawes Act) were in marked contrast to the theory of the legislation. Allotment, patent, and leasing opportunities resulted in the alienation of Indian lands and the deprivation of Indian agriculture. Drought, the advance of the market economy, the lack of capital and education, and finally depression did the rest of the damage (except for isolated examples of achievement).

John Collier, who became commissioner of Indian Affairs in 1933, confronted the failure of American Indian policy and offered an Indian New Deal. He believed that acculturation and assimilation had failed and advocated tribal sovereignty, communal ownership, agricultural planning, and federal support in finance and education. Not all Indians agreed. But, in 1945, Collier resigned under pressure of critics who charged that his policies promoted segregation and were injurious to the Indians. Assimilation and "termination" (that is, to end all relationships between the federal government and the Indians) again became federal policy. And the decline of Indian agriculture continued. In modern America, *Indian farmers, as true of most small farmers, lacked the capital and the land base to exist independently in the agricultural economy.*

The breadth of this work necessarily invites suggestions for inclusion from archaeologists, anthropologists, and historians. My own suggestion might be to strengthen the section on corn and tell readers more about pre-Columbian cotton. But, given the time frame, this is a nicely balanced book, and it is important to the scholarly community, government planners and policy makers, the informed public, and the American Indian community. The record of government-Indian land and agricultural policy has largely been one of failure, and there are precious few building blocks from the past on which to fashion viable programs

for the future, irrespective of one's advocacy of either acculturation or ethnic preservation.

HENRY C. DETHLOFF
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R. DAVID EDMUNDS. *Tecumseh and the Quest for Indian Leadership*. (Library of American Biography.) Boston: Little, Brown. 1984. Pp. viii, 246. \$14.95.

Early in the nineteenth century, Tecumseh, a Shawnee warrior leader, organized a multitribal alliance to halt the surge of settlers into the Old Northwest. For over a century the Shawnees had suffered from intertribal warfare and frontier expansion. Brushing aside British attempts to halt expansion, settlers began penetrating land south and north of the Ohio River.

The Shawnees and Tecumseh's family paid dearly for their resistance to British and American expansion. Tecumseh's father and two brothers were killed in frontier warfare. About one-third of the Shawnees, including Tecumseh's mother and a sister, migrated to southeastern Missouri. Tecumpease, Tecumseh's oldest sister, assumed responsibility in Ohio for eleven-year-old Tecumseh and four-year-old Tenskwatawa, Tecumseh's brother. Tecumseh and Tenskwatawa matured into remarkably different men; Tecumseh developed into a model Shawnee warrior and hunter, while Tenskwatawa, clumsy and fat, became an alcoholic known as Lalawethika (The Rattle or Noisemaker).

In April 1805 Tenskwatawa fell into such a deep trance that his family thought him dead. When Tenskwatawa revived, he claimed that he had died and traveled into the spirit world where the Master of Life revealed how Shawnees could prosper by resuming their traditional patterns of life. Subsequent visions filled out the teachings of Tenskwatawa (the Prophet), which were similar to those of other American Indian revitalization movements. Tenskwatawa condemned Americans as the children of the Evil Spirit or Great Serpent and as witches whose spells persuaded people to emulate whites. After some hesitation, the Shawnees accepted Tenskwatawa as a holy man, and his doctrines attracted converts from other Old Northwest tribes. From the Prophet's religious followers, Tecumseh began to form his alliance to resist American occupation of Indian lands.

Previous interpreters of the brothers' lives have minimized the Prophet's importance as a basis for Tecumseh's confederacy. R. David Edmunds asserts that "[o]nly after the Treaty of Fort Wayne [1809] did Tecumseh emerge as the dominant leader, and even then he based his political confederacy upon his brother's religious movement"

(p. 223). Tecumseh contended that he better represented the Old Northwest tribes than did "government chiefs" who possessed no authority to bargain away lands shared by all tribes. This stance brought Tecumseh and William Henry Harrison into confrontation, enhancing the Shawnee leader's visibility to historians.

After the Battle of Tippecanoe in 1811, Tecumseh's leadership continued while the Prophet's virtually disappeared. Disgraced by his failure to protect warriors from death, and avoiding battle as he had throughout his life, Tenskwatawa died in 1836 on the Shawnees' Kansas reservation. Tecumseh and his allies, however, joined the British during the War of 1812. He was killed at the Battle of the Thames in October 1813, refusing to retreat from Harrison's troops.

Edmunds strips away the apocrypha from Tecumseh's life. Yet the Shawnee leader's honesty, generosity to foes, and dedication to his people exemplify "the European and American concept of the 'noble savage,'" and the vagueness surrounding his death and burial has "only added to his mystique" (pp. 224–25). Drawing partially from his previous publications, Edmunds in this reinterpretation of Tecumseh and Tenskwatawa presents a most skillful and convincing analysis of their intertwined lives.

DONALD J. BERTHRONG
Purdue University

ROBERT WOOSTER. *The Military and United States Indian Policy, 1865–1903*. (Yale Western Americana Series, number 34.) New Haven: Yale University Press. 1988. Pp. xiii, 268. \$22.50.

Robert Wooster's book is a model of analytical history. In a little over two hundred pages of spare, cogent prose, Wooster delineates military strategy against the western tribes, places the political influence of the Gilded Age military establishment in solid perspective, gives an able survey of the institutional structure of the postwar army, briefly describes key Indian campaigns, and presents pithy characterizations of leading western military personalities. This is a wide-ranging, astute analysis of one of the most important institutions, if not the most important, on America's last frontier. Wooster's book places events in a national, and in military terms international, context. In so doing he has made a major contribution to frontier and military scholarship.

The author's mastery of a wide range of manuscript sources and secondary works gives a depth and maturity to this book rarely encountered in a monograph by so young a scholar. He has carefully incorporated the work of those who came

before—most notably Robert M. Utley and Francis Paul Prucha—and then goes beyond them to contribute significant new insights of his own to the history of the military frontier.

Occasionally, however, Wooster falls victim to his own conscious effort to be revisionist, as in his contention that the connection between Civil War and Indian war strategies of total warfare has been greatly exaggerated. He instead emphasizes key differences between the two types of conflict. Politics, logistics, race, and environment, Wooster contends, all "believe a direct correlation between the Indian wars and Civil War experience" (p. 140).

But the author's usually persuasive prose falters on this important point. He overestimates the influence of dissenters from the total war philosophy of Philip Sheridan and William T. Sherman, and he sometimes crucially underestimates the power wielded by those leading officers in formulating national policy. The concept of total war was taken much further on the frontier than it was during the Civil War, and Wooster attributes this to racism. He is partially correct, but it is also true that this brutal strategy was still in a developmental stage. In the twentieth century, even when race was not a factor, American military strategists proved even more brutal in their application of military power. Americans were brutalized by their frontier military heritage even more than Wooster is willing to contend and not just in the years covered by this admirable study. The roots of American reliance on concepts of total warfare go back long before the Civil War to over two centuries of bitter frontier fighting.

Other readers may well disagree with other points in this study, but all will agree that this is an important, intelligent, and even provocative book. It is further evidence of the superb scholarship being produced by students of both American frontier and military history. With the publication of this book, Wooster has emerged as one of the ablest and most promising young scholars at work in these lively fields of history.

PAUL ANDREW HUTTON
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ROBERT V. REMINI. *The Legacy of Andrew Jackson: Essays on Democracy, Indian Removal, and Slavery*. (Walter Lynwood Fleming Lectures in Southern History.) Baton Rouge: Louisiana State University Press. 1988. Pp. xii, 117. \$20.00.

Having previously published nine volumes (by my count) on the life and the presidency of Andrew Jackson, Robert V. Remini promises to "offer something different, something special" (p. 1) in

his latest discussion of this man he finds so fascinating. *The Legacy of Andrew Jackson* fulfills its author's promise. Although Remini unavoidably traverses ground he has covered many times in the past and the general lines of his interpretation will surprise few historians familiar with his work, the new book is indeed different from most scholarly studies and unusual, if not special.

Eschewing references to Jackson's "blazing" and "flashing" eyes or to bodies "sizzling with passion" and other characteristic Reminian stylistic touches and focusing as it does on interpretive issues and controversies a general reader might find somewhat arcane, the book appears to be directed at a scholarly audience. Such an audience might expect that the author's evident need to write yet again about a figure he has so many times discussed in the past signifies a ceaseless effort by a curious student to unearth new evidence on and attain deeper insight into someone of remarkable complexity, someone whose personal and political essence is tantalizingly elusive. In fact, Remini writes like someone who, knowing that Andrew Jackson was a great democratic "ideologue" dedicated above all to freedom, the needs of the poor, and rule by the "virtuous" and infallible majority, knows essentially all that he needs to know.

What is most special about this volume is a spirit of partisan advocacy that is surprising, coming as it does from a historian, and the number and vigor of its complaints against both historians and Jackson's political opponents. What is indeed fascinating is that Remini, who once wrote caustically of hypocritical, demagogic, and deceitful builders of the Jacksonian party and who responded sympathetically both to historical evidence and scholarly analyses at odds with his own viewpoints, has come to take on the qualities of Jackson when confronting critics.

Other historians are not simply wrong-headed. In not appreciating Jackson's devotion to freedom, equality, and democratic principle; in "refusing" to understand that he destroyed thousands of Indians, pilfered their lands, bribed their leaders, and drove them from lands they legally occupied out of "humanitarian" concern for preserving their culture and that, although he thought them unintelligent, avaricious, fickle, unreliable, treacherous, and bloodthirsty, he yet cared for them as his "children"; in not perceiving that the Jacksonians loathed, vilified, and urged violence against abolitionists not because Jacksonians were racist champions of slavery but because they were dedicated to preserving not only the Union but the democratic "impulse," historians are biased, prejudiced, unfair, even deaf as when, in hearing only "the sounds of an outraged egomaniac," they have failed to hear that all of Jack-

son's "political battles ring with a democratic cry" (pp. 20, 38, 83, 89, 90). At Remini's hands, John Quincy Adams's campaign against slavery becomes a mischievous exercise in hypocrisy.

I do not know whether Ralph Earl, the court painter who from Jackson's White House turned out portrait after portrait of the seventh president, followed Cromwell's advice to his portraitist. Certainly Remini's prose portrait concedes the equivalent of warts in Jackson's character, values, and actions. But, instead of pondering the implications of Jackson's lawless actions or of his repudiations of his alleged constant deference to the "will of the people," Remini strains to justify, rationalize, or put in the best possible light his hero's behavior. Remini does so typically by regaling us with what he claims are the lofty motives of Jackson and the Jacksonians, whether in brutalizing Indians and abolitionists, defending slavery, or violating American law in behalf of higher law. Of course, it is not clear that selfless motivation, even if proven, justifies governmental misbehavior or that the depth of sincerity, say, of an upstate New York lynch mob led by Jacksonian Democrats would detract much from the unloveliness of such a performance. Unfortunately, Remini, although quick to discount the lofty explanations offered by anti-Jacksonians for their actions, treats the self-serving justifications offered by Jackson and his followers as the "stated and real" reasons for their behavior (p. 87).

In a memorable exchange at the 1978 meeting of the Southern Historical Association in St. Louis, the eminent legal historian Mary K. Bonsteel Tachau, responding to Remini's criticism of her documented charge that Andrew Jackson believed himself above the law, observed that Andrew Jackson and Robert Remini deserve each other. One suspects that Remini is increasingly delighted with her observation.

EDWARD PESSEN

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ROBERT A. TRENNERT, JR. *The Phoenix Indian School: Forced Assimilation in Arizona, 1891-1935*. Norman: University of Oklahoma Press. 1988. Pp. xv, 256. \$22.95.

Founded in 1891, Phoenix Indian School, like a number of similar off-reservation institutions in the American West, became an important component in the reform crusade to assimilate American Indians. As Robert A. Trennert, Jr., stresses in his introduction, the school at Phoenix was modeled after famous Indian schools located at Carlisle,

Pennsylvania, and Hampton, Virginia. The philosophy behind all such institutions involved separating students from their tribal heritages and family influences, indoctrinating them in white values, and providing vocational training with a minimum of academic education. Although the Phoenix school and similar schools in the West affected thousands of Indian youngsters, they have received relatively little scholarly attention. Indeed, Trennert's book is a pioneer study of an important but badly neglected subject.

The author organizes his book around the various superintendents who presided over the Phoenix Indian School. Although all attempted to fulfill the assimilation mission prior to the 1930s, none succeeded fully. The famous "outing system" that the schools in Carlisle and Hampton used to place students in white homes and jobs, for example, became less a means of cultural reorientation at Phoenix than an employment agency to provide cheap labor for local white employers. Other factors that hampered assimilation included the inability of students to find decent jobs after graduation, chronic underfunding and overcrowding, severe health problems, and the devotion of so much of the youngsters' time to operating the school. As Trennert repeatedly stresses, superintendents managed to give the appearance of impressive gains, but real achievements usually fell far short of official claims. He particularly criticizes the neglect of academic training, which severely handicapped students who wished to continue their education.

The author's detailed coverage ends with 1935. For a decade or so, John Collier and other reformers had criticized the Phoenix school and similar schools for their assimilation philosophy, harsh discipline, health problems, and inadequate diet. Superintendent John B. Brown, who resigned in 1931, was the last of the authoritarian administrators who pursued earlier educational objectives, and by 1935 the school revamped its curriculum with a greater emphasis on academics and cultural pluralism.

Trennert deserves high marks for producing a well-crafted book. He uses a variety of local manuscript collections, materials from the National Archives and the Federal Records Center at Laguna Niguel, California, congressional hearings, and annual reports of government agencies. He combines those with oral interviews, newspapers, books, and articles. His well-organized narrative not only presents a clear account of the school's operations but also deals with national Indian policy and the sometimes important influences of the local white community. His assessments of the school are even-handed. If any weakness exists, it is Trennert's inability to gauge the impact that the

institution had on students. As he notes, the memoirs of graduates represent the more successful students, and little evidence exists for the mass of former students.

Despite this difficulty, Trennert's book is a significant contribution to Indian history, and it should serve as a model for future studies of other important off-reservation boarding schools.

DONALD L. PARMAN
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HERBERT SHAPIRO. *White Violence and Black Response: From Reconstruction to Montgomery*. Amherst: University of Massachusetts Press. 1988. Pp. xvi, 565. Cloth \$35.00, paper \$14.95.

The thesis of this important book is that subjection to violence has been a central feature of the Afro-American experience and that millions of blacks have always lived in the shadows of numerous episodes of racist brutality. Herbert Shapiro's main focus is on acts of direct physical violence intended to damage and destroy blacks. These acts have not been isolated instances of irrational behavior; they have been methodically used to construct and perpetuate white supremacy. During Reconstruction, white supremacists waged an informal civil war against any tendency toward racial egalitarianism, and they established a system of violence that has persisted well into the twentieth century. This system, sanctioned by majority public opinion and aided or abetted by governments at all levels, eventually permeated the nation and became ever more sophisticated with the adaptation of technology to violence.

The most characteristic forms of violence have been lynchings and mob assaults against communities of blacks. Shapiro offers a perceptive analysis of the genesis, composition, and behavior of a host of notorious and obscure mobs. He provides ample and damning evidence to show that violence against black communities was often comparable to European pogroms and that, well into the twentieth century, lynchings were almost commonplace in the South. Legal lynchings complemented the mob, and scores of blacks were executed as a consequence of this perverted form of justice.

Shapiro convincingly argues that the nation's willingness to use violence abroad has been accompanied by increased racial terror at home. During the 1950s militant cold war anticommunism merged with racial hatred to accelerate attacks on blacks, and violence intensified as racists resorted to widespread terror to maintain the status quo in the wake of the *Brown* decision.

In his extensive treatment of the response to

violence, Shapiro shows that blacks have employed a variety of strategies in their consistent and determined struggle for security. The most fervent and unanimous response was their demand for federal antilynching legislation. Accommodationists sought protection from the best class of whites within the southern tradition of paternalism, and white violence drove the ideology of black nationalists and separatists. Integrationists, often in alliance with liberal whites, linked physical violence with other forms of racial oppression in their insistence on full equality. Other blacks acted on the premise that retaliatory violence was an appropriate response to racial terror.

In the interval between the two world wars, the black response moved to new levels, and blacks showed a greater propensity to resist violence with violence. Issues of class and race were more prominent than ever before, and an increasing number of blacks saw salvation in working-class solidarity. Others turned to revolutionary socialism and communism, which offered a means of responding to violence through international organizations. After World War II, blacks petitioned the United Nations for relief from oppression. Shapiro sees Martin Luther King, Jr., as the leader who developed an appropriate strategy to counter racial violence. King placed the black struggle at home within the mainstream of the international movement for the liberation of oppressed people, and he created a mass resistance movement that was "clothed in the disarmingly appealing garb of love, forgiveness, and passive resistance" (p. 431). Although radical, King's strategy was in consonance with the American democratic tradition, the forces of law and order, and the basic ideas of humanism.

Besides being an important commentary on violence, this book is a splendid contribution to American history, and it deserves praise for its comprehensive and sensitive treatment of a topic that many would like to avoid. By taking the reader through the maelstrom and horrors of the black experience since the Civil War, the book provides a greater understanding of the pathological nature of racism and the profound contradictions between our national ideals and the realities of American society. It also helps dispel the myth that violence has been merely tangential to our national experience.

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MARGARET WASHINGTON CREEL. *"A Peculiar People": Slave Religion and Community-Culture among the Gullahs.* (American Social Experience Series, number 7.) New York: New York University Press; distrib-

uted by Columbia University Press, New York. 1988. Pp. xv, 417. \$40.00.

In this most promising first book, Margaret Washington Creel takes a very close look at Christianity and culture among African-American slaves. Her title is derived from the biblical verse "ye are a chosen generation, a royal priesthood, a holy nation, a peculiar people that ye should show forth the praises of him who hath called you out of darkness into his marvelous light" (1 Pet. 2:9). Her intelligent and provocative study is the result of her concern with what she calls "the sociohistorical relationship between community, religion, and resistance as these concepts affected African-Americans during slavery" (p. 1).

There is nothing quite like this book in the literature either of African-American slavery or of African-American religion. Creel excludes Gullah-speaking Georgians from her study and instead focuses on the Gullah-speaking people of the South Carolina coastal region. Within the narrow geographical range, however, her approach is anything but limited. After an essentially historiographical introduction, she turns to the formidable task of summarizing the African background of Gullah religion, missionary activity, slave conversion, the Great Awakening, the religious instruction of slaves, the emergence of independent black churches in the lowcountry, and the Methodist and Baptist denominations among slaves. She also devotes chapters to the Gullah synthesis of African and European religious elements, Gullah attitudes toward death and the supernatural, and the relation of slave religion to slave resistance. In addition, she includes appendixes on the ethnic origins of South Carolina slaves and on Gullah folkways.

The religion of Gullah-speaking slaves in the lowcountry of South Carolina was more than a darker version of white Christianity. The slaves blended the religious heritage of Africa with that of Europe to create Afro-Christianity, a unique synthesis with unique styles. Although the new religion was clearly Christian in theology, the manner in which slave Christians worshipped God presented a striking contrast to the passive and sedate services of European Christians. Creel attempts "to strike a balance between 'pure' religion (for its own sake, other worldly, or transcendental) and applied religion (its functional or empirical use) because both were practiced among slaves who ultimately adopted Christianity" (p. 3). This astute distinction is not, however, developed in detail.

Creel's conclusions regarding Gullah religion do not represent major revisions of the findings of Albert Raboteau, Lawrence W. Levine, and Vin-

cent Harding, although she puts greater emphasis on resistance and day-to-day religious expression. In many ways this study fits comfortably within the category described by Peter Kolchin as the "community and culture school." Fortunately, Creel's sensitivity to context enables her to avoid, for the most part, what Kolchin considers an excessive propensity toward sentimentality on the part of such scholars.

Moreover, unlike some "community and culture" scholars, Creel takes pains to define what she means by the terms "community" and "culture." She sees community as something that "occurs when a group of social actors are bound by responsibility, accountability, and compromises made in order to maintain organization and stability." She "presupposes that subjectively the term represents a sense of collectivity and togetherness, based on a definite pattern of essentially positive relationships, possibly changing over time, and a sense of mutual dependence." In exchange for "the closeness and security which community offers, it extracts loyalty, obedience, relinquishing of some individual freedom, and personal authority." She defines culture as "creative, adaptive, dynamic patterns of behavioral meanings, which are inherited and historically transmitted through rituals, symbols, and systems of communication that represent societal understanding, awareness, and conceptions of life" (p. 1).

This is a powerful book. Although Creel's narrative is rather dry, her treatment is very thorough, and her evidence is effectively marshaled. Relying on records of the Society for the Propagation of the Gospel and documents from various denominations, ex-slave narratives, memoirs, manuscript correspondence, contemporary periodicals, and court records, she has painstakingly pieced together her story from scattered sources. Creel's command of the literature of African ethnohistory is the most impressive of any historian thus far writing on slavery. Her discussion of belief systems is informed by sophisticated anthropological theory. Her treatment of African influences on Gullah culture as dynamic continuities rather than as static "survivals" reflects a great advance beyond the older Herskovitsian model.

Creel's major analytical modes are comparative and dialectical. In choosing a comparative approach for Gullah relations with African religions and a dialectical approach to Gullah relations with white Protestantism, however, she begs the question, virtually guaranteeing the conclusions at which she arrives. Her argument is for the most part persuasive. But it is hardly surprising, under the circumstances, that she finds mainly African continuities with some white Protestant influence.

Despite prodigious research, her contentions

are not always convincing. For instance, not all readers will agree that Gullah concepts of community and aspects of spirituality derive specifically from secret societies in what are now Sierra Leone and Liberia or that Gullah age-sex religious societies can be traced to such secret societies. And her treatment of Gullah religion as a form of resistance may strike some readers as strained, although she dodges this criticism somewhat by redefining "resistance" in broader terms.

It is unfortunate that Creel's command of the linguistic scholarship is not as strong as her linguistic pronouncements. She depicts Gullah as a "true dialect" (pp. 96-99). It is in fact a creole language created by enslaved Africans from the convergence of various African and European linguistic features. A more accurate understanding of the Gullah language would have strengthened her major argument regarding Gullah religion. Another problem is her shaky command of Gullah geography. She describes "the land of the Gullah slave population" as being "bounded on the north by Georgetown, and on the south by Port Royal and St. Helena Sound." Yet she includes a long appendix (pp. 344-55) of Gullah folkways from All Saints Parish, an area north of Georgetown with a heavy concentration of Gullah slaves.

Nevertheless, Creel has written the fullest and most valuable account yet of the religious life of the Gullah-speaking slaves of the South Carolina lowcountry. It is a very impressive study.

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WILLIAM D. PIERSEN. *Black Yankees: The Development of an Afro-American Subculture in Eighteenth-Century New England*. Amherst: University of Massachusetts Press. 1988. Pp. xii, 237. Cloth \$25.00, paper \$11.95.

Eighteenth-century New England would hardly seem to have been fertile ground for the survival of African folkways. Black bondsmen and bondswomen were vastly outnumbered by settlers of English descent and marooned in an alien culture. William D. Piersen contends, however, that few of the region's slaves were transformed into "black Yankees." Continued importation meant that an African presence persisted for decades. Moreover, many slaves actively resisted assimilation, refusing to abandon their cultural heritage or to regard New England as home, while whites contributed to African survivals by excluding blacks, slave and free alike, from full membership in the commu-

nity and by tolerating forms of behavior that did not appear to challenge established authority.

Emphasizing humble acceptance of one's lot and insisting on a sound knowledge of theology, rather than on a conversion experience, as a prerequisite for church membership, Yankee ministers made few converts among the slaves. Although during the Great Awakening some preachers attempted to reach out to the slaves, this was a short-lived experiment and, so Piersen suggests, not entirely welcomed by masters who feared that "regenerate" slaves would refuse to obey those they considered unrepentant sinners. The majority of the region's slaves were left to their own religious practices—practices that irritated whites but were not perceived as threatening.

Other cultural patterns survived or were retained in modified form. In work techniques, stories, music, and the use of satire, there was much that was distinctly African. Black election day—celebrated in many New England communities—was not an attempt to imitate white political life but an African tradition that flourished in slave cultures elsewhere in the Americas. Whites occasionally expressed alarm at the gathering of large numbers of slaves, some of them armed, to choose kings or governors for the following year, but masters continued to lend their slaves clothes, horses, swords, and guns and to advance money for election campaigns.

Piersen concedes that some readers may be uneasy about the kind of evidence he relies on in his study (pp. xi–xii). With a background in both history and folklore, he draws on material that has generally been ignored by scholars who lack his interdisciplinary training. Anecdotes, many of them "reflect[ing] a crudely patronizing if not racist tone" (p. xii), are mined for what they reveal both about slave culture and about white perceptions of that culture, while cross-cultural data are employed to illuminate many aspects of the New England slave experience.

A number of points deserve clarification or further discussion. Piersen notes that black governors and their deputies wielded power within the slave community that was far from symbolic. How far, if at all, was white authority served by the creation of this informal power structure? More might have been said about American Indian influences on New England's black inhabitants, although the issue is not ignored by the author. On the nature of the New England slave trade, it is clear that small-scale slave traders "had little need to gamble cruelly on purchasing large cargoes of African slaves with their high mortality rates" (p. 3), but that is hardly true of the Browns of Providence and their rivals, who engaged in

extensive slaving enterprises. Although the Caribbean was "a way station on the transatlantic trade" (p. 6), rather than a source of slaves, numbers of slaves destined for sale in New England spent months, and sometimes years, in the Caribbean. In terms of their response to New England slavery, what lessons about enslavement and Euro-American culture had they already learned in their sojourn in the West Indies?

Minor criticisms apart, this book is a provocative and finely crafted study of the inner world of New England's bondsmen and bondswomen.

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GARY B. NASH. *Forging Freedom: The Formation of Philadelphia's Black Community, 1720–1840*. Cambridge: Harvard University Press. 1988. Pp. xii, 354. \$28.95.

Gary B. Nash's book is a major contribution to our understanding of black life in the early American republic; it is a vivid and compelling account of the evolution of Philadelphia's black community in a period of increasing racism.

The book opens with an interracial gathering celebrating the construction of Philadelphia's African Church in 1793. It ends with a violent assault on the black community by Philadelphia whites in 1834 and the burning of Pennsylvania Hall, a symbol of abolitionism, four years later. In those forty-five years, a city in which many whites seemed committed to racial harmony became a center of militant racism. But, simultaneously, the free black community not only increased from two thousand to fourteen thousand five hundred (remaining throughout the period about 10 percent of Philadelphia's population) but also became a vibrant community, a thriving network of families, churches, schools, and fraternal orders. "Tragedy and triumph" (p. 6) are Nash's themes: the tragedy of the failure of racial justice and the triumph of the black community in the face of increasingly adverse odds.

Drawing on his unmatched knowledge of Philadelphia's history in the late eighteenth and early nineteenth centuries and on a rich array of sources including church records, ships' crew lists, and coroners' reports, Nash handles the second of those themes with consummate skill. He traces the emergence of black institutions even as the black community was transformed by an influx of freed slaves from the Pennsylvania countryside and then by men and women escaping bondage and the deteriorating condition of free blacks in the South. He makes clear that, despite economic discrimina-

tion (notably their exclusion from the city's growing industrial sector), blacks carved out a niche at nearly all levels of the class structure. Most worked as unskilled laborers and servants, but by the 1820s substantial numbers had achieved economic independence as professionals, shopkeepers, and artisans.

At the same time, the black community was being Americanized. Nash analyzes naming patterns of the postrevolutionary generation to show how blacks both threw off the legacy of slavery and moved closer to Anglo-American culture. By the 1790s, with slavery in Pennsylvania all but dead, blacks had abandoned surnames of former masters and first names of African and classical origin, choosing common American names for themselves. And Philadelphia blacks did not show any interest in emigration. In 1817 the city was the site of the first black convention to denounce the American Colonization Society. Although a few prominent community leaders were attracted to colonization, one reported that "there was not a soul that was in favor of going to Africa" (p. 238) among the ordinary blacks who made up the gathering.

Successful in depicting the rise of the black community, Nash falters when he tries to explain the increasingly virulent racism of early nineteenth-century Philadelphia. He offers a number of explanations, none of them entirely convincing: the growing number of poor and criminal blacks, the influx of alien ex-slaves from the South, fear of black rebelliousness after the revolution in Santo Domingo, job competition between Irish immigrants and blacks (even though his evidence shows that blacks, not immigrants, suffered from such competition, as the Irish increasingly monopolized unskilled jobs traditionally performed by blacks). In the end, Nash seems to view racism as a kind of misunderstanding: whites adopted an inaccurate "mental picture of black society" (p. 275), seeing only destitution, criminality, and intemperance where in fact a flourishing, complex community existed.

Although Nash notes that wealthy and prominent Philadelphians were no less racist than the immigrant poor, the book contains no discussion of the web of commercial and financial connections that tied the city's upper class to the South's expanding slave economy. There is nothing on the politics of race or on the emergence in this period of a powerful nationalism closely tied to a racial definition of what it meant to be American. To develop a convincing explanation for racism, in other words, one must look beyond the category of race relations and beyond a single city.

Nonetheless, this book deserves a wide audience both for its sympathetic portrait of an urban black

community and for its timeliness. Nash's study ends in 1838, but a glance at today's newspapers suggests that our northern cities have not progressed very far since then toward realizing the dream of an integrated, just society.

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GEORGE SELEMENT. *Keepers of the Vineyard: The Puritan Ministry and Collective Culture in Colonial New England*. Lanham, Md.: University Press of America. 1984. Pp. vi, 122. Cloth \$19.75, paper \$8.75.

Intellectual historians have stressed Puritanism's impact on New England whereas social historians have minimized it, George Selement avers, but both groups agree that ministers, whether immersed in scholarly arcana or clinging to outmoded ideals, were isolated from the people. Selement himself hopes to assess religion's hold on the region by showing that the clergy's commitment to pastoral care drew them close to their flocks and fostered a collective culture. Puritan ministers educated the young, counseled the confused, succored the sick, and aided the poor. They carried the Word far beyond their own little bands of Saints, walking the neighborhoods to contact unregenerates, proclaiming the gospel to blacks and Amerindians, taking up pulpits elsewhere in the colonies, and supporting missions worldwide. Aiming their prodigious literary output at a mass audience, they pressed their tracts into as many hands as they could; illiterates benefited by hearing others read. Attentive, evangelical, communicative, and dedicated, ministers disseminated their values throughout the populace, forging a common mentality. In time, however, heterodoxy, materialism, and moral laxity ballooned despite the clergy's escalating catechetical efforts, gradually eroding the consensus. Some ingredients of the common mentality survived into the eighteenth century, but by 1700 Puritan collective culture had essentially given way to Yankee society.

Selement ably suggests that historians have mis-cast Puritan ministers as cloistered academics concerned only for their tribe of church members. The preachers were, he proves, engaged pastors and active proselytizers, but his arguments suffer from factual, logical, interpretive, and conceptual imprecision. Most important, he neither specifies what ideas and practices composed the "collective culture" nor fully appreciates its popular elements. Focused on the clergy, he both overlooks the interactive process by which the laity helped shape their world and homogenizes differences between elite and folk variants of shared beliefs.

Clerics and parishioners accepted the reality of witchcraft, for example, but disagreed about the crime's significance and appropriate legal remedies. By defining "collective culture" merely as "doctrine and piety" (p. 79), he reduces it to an exclusively religious term that frustrates his broader intention to establish the "existence of a Puritan New England" (p. 9). No one denies that Puritanism dominated the region's faith, but to demonstrate its larger influence necessitates discussing how much it shaped other spheres of life. Selement is right in sensing that Puritans forged a cultural hegemony in New England, but one wishes he had made a better case for their success.

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MICHAEL G. HALL. *The Last American Puritan: The Life of Increase Mather, 1639–1723*. Middletown, Conn.: Wesleyan University Press. 1988. Pp. xv, 438. \$35.00.

Michael G. Hall's account of Increase Mather is a straightforward narrative of the life-and-times genre. The author knows his subject inside out, having studied it for a quarter-century; he is conceivably the only person since Mather himself to have read every word of Mather's fifty-year-long diary, published for the first time by Hall in 1961. Yet this engagingly written book wears its learning lightly; it can afford this modesty because the author builds on the deep foundation of Puritan and New England studies to which his notes make occasional, if understated, reference. One way to begin to measure the magnitude of that foundation—and thereby to appreciate Hall's contribution—is to compare this book with Kenneth B. Murdock's pioneering *Increase Mather: The Foremost American Puritan* (1926), an impressively erudite work in its own right. Yet Hall stands less in the intellectual history tradition of Murdock, Perry Miller, and Robert Middlekauff than in the line of historiographic descent from the Samuel Eliot Morison of *Builders of the Bay Colony* (1930).

Son of a builder, son-in-law of another, a maintainer and renovator himself, Mather was unarguably the chief clergyman of his generation in New England. Minister at Boston's Second Church for nearly sixty years; head of Harvard for sixteen; Bay Colony agent to England in the charter crisis; ever active in shaping and reshaping the New England church way; a Jeremiah of Jeremiahs; surpassed only by his logomaniacal son as a producer of the printed word, he was also, arguably, the chief public figure—bar none—of his period and place. Only the fact that he was characteristi-

cally embattled—as often dissident as dominant—keeps one from speaking of the Age of Increase Mather. Hall makes the case for Mather's focality persuasive simply by telling Mather's story.

For Murdock, Mather was the "foremost" American Puritan; for Hall, he is also the "last." That is as it may be: a friend once cautioned me never to gamble scholarly credit on a claim to know the first or last of anything; some smug know-it-all will invariably come up with something earlier or later. Hall's point—it is not hard driven enough to be called a thesis—is that Puritanism peaked in the 1670s, that the fading of Mather's public prestige toward the close of the century was a sign of greatly changing times, and that Mather's inner world also changed. He learned to speak Whig (liberty and property) as a second language, alternative to Puritan, his mother tongue.

Mather's lastness is especially the theme of Hall's concluding chapter, where, after 1692, the story line turns downward. Mather still had thirty years to go, but he became more and more marginal: the life and the times diverged. Eruptions of liberalism at Brattle Street Church and Harvard, the flourishing of trade and traders, the parting of the ways of church and state under the new charter, the tightening grip of imperial administration—all signified the passing of Mather's age. Departing like New England's glory, Mather himself finally ceased to exploit the legend he had helped invent—the legend of transcendent errand and ancestors—in "tacit recognition" that "Boston was no longer Immanuel's land" (p. 353) or likely ever to become so.

Although keyed to central themes and issues in American Puritan studies, Hall's biography parades no grand generalities, major interpretive revisions, or deep thoughts on the meaning of it all. Neither does the book set the stage of Mather's life with many props from the cabinet of social history. Nor does it try to win prizes for Mather as a profound or constructive theologian. But what it does, it does very well, and the well-told tale is graced with the kind of insights into Mather's character and notes on his performance that only close acquaintance with the man and his era can generate. Increase Mather has been twice blessed in his modern biographers.

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SALLY SCHWARTZ. *"A Mixed Multitude": The Struggle for Toleration in Colonial Pennsylvania*. (The American Social Experience Series, number 8.) New York: New York University Press. 1988. Pp. viii, 399. \$45.00.

William Penn's ideas about religious tolerance at the time of settlement set the pattern for colonial Pennsylvania despite some challenges. This is Sally Schwartz's major thesis. Because Penn did not want to encourage emigration from England, lest it weaken the homeland, he turned instead to the Continent—Germany, Holland, and France. His translated pamphlets were successful in attracting colonists. He chose to sell land to wealthy individuals or companies; they hoped for autonomy and isolation but did not get much. He did not realize that, to the pioneers, the promise of "security of property might be as important as liberty of conscience." He knew he was "acquiring people of all sorts of nations and persuasions" but did not "appear to have thought clearly about the implications of his efforts to recruit alien immigrants" (pp. 26–27). He defended religious nonconformists but not political dissidents. The phrase "ideology of tolerance," in the epilogue (p. 297), seems too strong a description of his ideas considering the vagueness and inconsistency of Penn and the colony's early policies.

Schwartz indicates in several places that the differences in Pennsylvania were religious, not ethnic, and that they were mostly within, rather than between, religious groups. It was important that the German pioneers were sectarians, not "church people," that is, Lutheran or Reformed. The "church people" were accustomed to established status, whereas the sectarians were a persecuted minority that had much in common with the Quakers. In Pennsylvania these sectarians proved to be faithful allies of the Quakers on political and social issues.

When the "church people" arrived later, they tried to use their education and status to proselytize, but the sectarian pattern of tolerance was too far along, and they had to pull back. Anglicans also longed for establishment in Pennsylvania and an American bishop but could not be as aggressive as they were in other northern colonies.

Although Schwartz's picture of tolerance is attractive, it must be reconciled with those emphasizing the continual friction between the Scots-Irish Presbyterians and the large Quaker-German group in the province. Robert Kelley, in *The Cultural Pattern of American Politics* (1979), declares bluntly that "the arrival of the Scots-Irish . . . produced a political earthquake that shifted the whole landscape" (p. 71). His sources are James Leyburn, James Hutson, and Owen Ireland. William Nelson, in *The American Tory* (1961), also found polarization. Germans tilted toward the Tory side in the revolution primarily because they feared that the Spirit of '76 would bring the Scots-Irish to power in Pennsylvania. The Presbyterians' dogmatism might deny to others the liberty that the

revolution was promising. Although Schwartz notes in passing the contentiousness of the Scots-Irish, she minimizes their role and conflict with others.

Schwartz's use of quotations is impressive, and she has retained the original spellings. Even scholarly readers may be slowed, however, by three digit numbers in the middle of sentences. There are 1,602 footnotes for a text of 302 pages.

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PAUL A. GILJE. *The Road to Mobocracy: Popular Disorder in New York City, 1763–1834*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1987. Pp. xvi, 315. Cloth \$32.50, paper \$9.95.

Paul A. Gilje has interpreted the changing meaning of collective disorder in New York City from the era of the American revolution to the Age of Jackson. Writing in the tradition of E. P. Thompson and George Rudé, Gilje identifies general shifts through the period in the social composition of riotous crowds, their targets and victims, the methods and levels of violence they employed, and the tolerance afforded them by the respectable classes. Gilje explains that changes in these indicators reflect changes in the structure of New York's (and America's) economy and society.

Gilje argues that the reigning ideological ethos of eighteenth-century New York City stressed "corporate communalism" (p. vii) and that the riots of the revolutionary era and early nineteenth century helped preserve and extend solidarity among white Protestant native-born Americans of all social classes. Riots such as the annual eruptions on Pope Day or those against the Stamp Act reinforced the sense of unity that existed between upper and lower orders by focusing on mutual enemies such as Catholics and British tax collectors. Riotous crowds often included a few members of the upper and middle classes. Their targets were often effigies and symbolic representations of the offending parties rather than the offenders themselves. Levels of violence were limited, and few victims were killed. Rarely was the watch or the militia called to quell the rioting. With some exceptions, then, "the people out of doors" in eighteenth- and early nineteenth-century New York City expressed a broad majoritarian consensus.

A shift in the focus and methods of rioting in the second decade of the nineteenth century, however, reflected widening schisms in society. Increasingly, the targets of violence were ethnic and racial minorities: immigrants from Ireland

and Germany or runaway slaves and free blacks. The growing diversity of New York's population brought out ugly impulses in the native white working classes. By the 1820s, rioters displayed the intention to injure their victims directly, not simply send them a message by attacking a symbolic representation of them. Ethnic and black neighborhoods were terrorized by arsonists and rioters, and respectable members of society ceased participating in these more violent episodes. Rioting thus split the upper from the lower orders and pitted those at the bottom against each other—causing fear and discomfort for those at the top.

By the 1820s, Gilje argues, collective violence in New York City had become a primary indicator of class conflict. The book's final section focuses on labor actions, strikes, and violence among seamen, dockworkers, mechanics, artisans, and journeymen and on the appearance of working-class youth gangs and fire companies. Gilje also traces the solidification of a middle- and upper-class consensus against the rising tide of disorder and concludes his work with an account of the creation of the New York Police Department, which played such a large part in quelling the draft riots of 1863.

Gilje's study of rioting in New York City reinforces much that has been argued by labor and social historians since the 1960s. This book can take its place with those works of American social history that aspire to give a voice to the "inarticulate," those who speak to us through the record of their deeds rather than their diaries. Perhaps the next generation of American social historians will try to recapture the voices of those who were victims of these riots, articulating and interpreting the experience of victimization. Such an effort would be a welcome addition to our understanding of nineteenth-century rioting, which too many historians describe as if it were nothing more than an alternative way by which the masses communicated with the classes.

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ROBERT E. CRAY, JR. *Paupers and Poor Relief in New York City and Its Rural Environs, 1700–1830*. Philadelphia: Temple University Press. 1988. Pp. xii, 269. \$34.95.

The key to the success of this book is its scope; Robert E. Cray, Jr., writes about paupers and poor relief in both the city—New York—and the countryside—Westchester, Queens, and Suffolk counties. The approach is logical. The poor, as the author points out, were often mobile and could thus move between the two environments. Moreover, trade and other types of contact continually

flowed between urban and rural areas. The approach also has the benefits of a local study, while containing enough information for some comparisons.

The result is an examination of two areas that allows us to rethink our assumptions about the development of poor relief policy in the eighteenth and early nineteenth centuries. Poor relief policy did not move in a straight linear direction from outdoor community assistance to institutionalization. In New York City an institution for the poor emerged in 1735 and remained in place thereafter. But outdoor assistance continued throughout the period that Cray studies. The countryside did not lag far behind the city in the development of institutions. During the 1770s and 1780s, largely in response to problems created by the revolutionary war, many rural communities built their own almshouses, only to close them down in the 1790s when they returned to a reliance on outdoor relief.

Cray is also concerned with the human side of history. He effectively demonstrates that the decisions concerning poor relief were dictated by personal and local circumstances. More important, he attempts to show how the poor responded to public assistance and describes, as far as the difficult sources allow, the lives of the paupers in the communities he studied.

There are some shortcomings to this book. Repeatedly, Cray emphasizes economics as the reason for changing poor relief policy. Occasionally he writes about moral and religious sensibilities as affecting decisions, but there is no effort to deal systematically with the larger intellectual changes in the development of institutions for so-called social misfits covered by David Rothman (whom Cray criticizes in the introduction) and Michel Foucault (who is not even cited). And Cray does not fully explore the changing nature of social relations, although at times there are hints, that altered the interaction between paupers and those who controlled local coffers. Detail on those social changes, as well as some effort to examine the relationship between larger intellectual forces and local decisions, would have added greatly to the significance of Cray's work and would have made a good book even better.

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ROBIN F. A. FABEL. *The Economy of British West Florida, 1763–1783*. Tuscaloosa: University of Alabama Press. 1988. Pp. ix, 296. \$26.95.

This little volume succeeds in one of its professed purposes: to supplement the standard works on

the subject published in the 1940s by Cecil Johnson and Clinton N. Howard. In addition to the predictable official and printed sources, Robin F. A. Fabel has examined the land records in the National Archives and a wide range of colonial newspapers. Surviving statistics are limited, and the book features numerous sketches of merchants, officeholders, planters, and shipmasters. The author provides new and intriguing evidence concerning the colonizing exploits of the Connecticut-led Company of Military Adventurers. There is new detail on the mail packet boats to West Florida, but this information is not related to the economics of oceanic communications. There is a chapter on the origins, prices, and occupations of slaves in a colony looking for a staple commodity, but Fabel does not see the significance of the findings within the contentious historiography of slavery in this generation.

The more ambitious purpose of the book, to examine whether the people and colony of British West Florida were successful, seems more problematic. This is essentially a semantic question, which could not be answered by historical evidence even if the statistics were adequate. A twelve-year-old plantation colony of six thousand inhabitants, which was crippled by the revolutionary war and destroyed by Spanish conquest before a staple crop of consequence was developed, makes a poor case study in development economics. Fabel rightly insists that comparisons be made with other colonies of the same age but confines his comparisons to Georgia. He sees trade imbalance and outside credit as serious weaknesses rather than vital initial investment. Bills of exchange are also regarded as detrimental, causing inflation rather than liquefying a frontier economy. Despite these views, and the impossibility of measuring the scale of trade with Spanish Louisiana, Fabel offers a comparatively optimistic answer to his question.

The author has assembled and adequately indexed considerable research on various aspects of West Florida's brief history, which will be useful to its devotees. This work, however, would have benefited from thorough criticism by a copy editor and professional demands by scholarly appraisers. Scholars interested in the economic and social history of colonies and empires can find some intriguing evidence here that invites comparative analysis. Regrettably, this rather anecdotal and myopic study has ignored the methods and analytical questions that animate the study of early modern immigration, slavery, trade, and colonial economic development.

IAN K. STEELE
University of Western Ontario

ELIZABETH BARRETT GOULD. *From Fort to Port: An Architectural History of Mobile, Alabama, 1711–1918*. Tuscaloosa: University of Alabama Press. 1988. Pp. x, 317. \$39.95.

In this finely detailed history of Mobile architecture, the author traces basic building types, such as religious, commercial, governmental, and residential, from the early eighteenth century to 1918. By necessity, this study is also an economic history of Alabama's gulf city. During prosperous times private individuals, businesses, and governments built. During times of panic and depression, construction stood still.

Mobile was founded by the French in 1711, turned over to the English in 1763, taken by the Spanish in 1780, and claimed by the United States in 1813. The only continuing presence was Fort Condé, constructed by the French in the 1720s. While calling it a "venerable monument," in 1818 the Alabama legislature ordered the fort's destruction because it "obstructed our most interesting views" (p. 274, n. 17). This idea of destruction becomes a second theme for Elizabeth Barrett Gould's book as she tells of building after building demolished in the name of "progress." As she says, "None of the colonial buildings of Mobile has survived. Fire, hurricane, and man have destroyed them all" (p. 1).

Throughout most of Mobile's architectural history, the city followed the general styles popular in other sections of the country, adapting them to meet Mobile's particular needs of location and climate. Although Georgian and Federalist forms were brought to the city by early American settlers, they failed to adapt well to the semitropical climate.

Mobile both became a city and gained its first impressive buildings in the decade of the 1830s. Four large structures, all built on a floating-raft foundation and all in Greek Revival style, showed the booming prosperity of the Alabama cotton economy. The Old City Hospital of 1833, a monumental building with three-story Doric columns supporting open galleries on each floor, came first; the other three buildings marked a more sensitive appreciation of the style. James Gallier, Sr., and his associate, Charles B. Dakin, arrived in the city in the mid-1830s. Both had training and experience in the New York office of Town and Davis, and, although they spent only a few years in Mobile before establishing practices in New Orleans, their designs for Barton Academy, Government Street Presbyterian Church, and the Government Street Hotel revolutionized city architecture.

Gould calls the decade of the 1850s Mobile's golden age. Cotton was king, and rows of ware-

houses, stores, and offices solidly lined the streets of the inner city. The streets of the suburbs gained the residences, from cottage to mansion, to support the commercial activity. Even the coming of the Civil War failed to end construction completely, as Mobile became one of the major ports of the Confederacy.

The most distinctive Mobile architectural form is the Gulf Coast cottage. A marriage of the French Canadian farmhouse with the galleried dwelling of the Caribbean, it produced an adaptation of the familiar with a form better suited to the climate. The galleries protected walls from torrential rains and the hot sun. This union created a roof line with a broken pitch between a steep central section covering the house proper and the flaring lower section covering the porch.

The cottage continually developed with each new stylistic change. In the Federal period it lost its side galleries and broken-pitched roof line and gained a center hall and green shutters. The Greek Revival brought round tapered columns with a slight entasis. In the 1860s and 1870s came board-and-batten siding, decorative barge boards, turned columns, and brackets of the Carpenter Gothic. Although the form produced large and elegantly proportioned villas such as the Marshall Hixon house, (ca. 1853), by the end of the century it evolved into the plain dwelling of the working class.

Several times in this work Gould mentions the streetscapes and squares that create the unique charm of Mobile. Unfortunately, her photographs are almost entirely of individual structures isolated from their neighbors. Little of the delightful quality of the city is shown. Nonetheless, this scholarly and well-documented work is a very welcome addition to the literature.

DONALD W. CURL
Florida Atlantic University

STUART GALISHOFF. *Newark: The Nation's Unhealthiest City, 1832-1895*. New Brunswick, N.J.: Rutgers University Press. 1988. Pp. xii, 260. \$38.00.

In *Safeguarding the Public Health: Newark, 1895-1918* (1975), Stuart Galishoff dealt with public health problems and the responses of Newark from the 1890s to the end of World War I. That study focused primarily on the rise of scientific public health work in the era of the bacteriological revolution, on specific measures directed against individual infectious diseases, an approach that largely superseded earlier emphases on generalized environmental sanitation.

Galishoff has now turned his attention effectively to Newark's bad old days, to the city's public

health in the sixty-five-year period just before the advent of bacteriology. In the present volume he describes with rich detail the mid-nineteenth-century evolution of Newark from a small city of eleven thousand people into a major industrial city and from a reasonably salubrious community to one experiencing a disastrous deterioration of its sanitary arrangements, quality of life, and mortality rates. A large part of the work is devoted to examining the sporadic and never-adequate attempts of Newark's leadership to build up a "sanitary infrastructure" (p. 4) to cope with the city's enormous wastes and sources of pollution. This principally meant a network of drains and sewers, a water supply, and garbage disposal arrangements. The author also discusses the halting evolution of a city board of health and its continuing efforts to control smallpox through vaccination programs and quarantine. One chapter is devoted to the impact of cholera.

This study is thorough, wide-ranging, and well informed. It will be of interest to general and urban historians as well as to historians of medicine and public health. Commendably, Galishoff has not limited his inquiry to the proceedings of the board of health, a body that then had little authority or effectiveness. He has gone on to examine the sanitary activities of Newark's other municipal boards and agencies as well as the involvement of nongovernmental entities. He also explores the various social, ethnic, and class reactions to public health projects, the rapid increases in individual as well as industrial demands for sanitary services, the resistance of all groups to the costs, and the tedious political struggles that preceded any action. He emphasizes the low regard that mid-nineteenth-century upper classes tended to have for the lives and health of the lower classes. And he rightly shows that Newark, in all of its squalor and for all of the delays, the underfinancing, and the patchwork nature of its public health measures, differed little from most other American cities of the period.

Galishoff has constructed this book from an extensive mining of official reports, newspaper accounts, and secondary histories of Newark. He has also gone to great pains to use numerous special studies of American public health history. His extended recapitulations from those works of national conditions—they make up one-third or more of some chapters—are useful, but they are also sources of difficulty. They do enrich and provide a broad context for his account of local developments. And they remind one of the long-existing need for a comprehensive, comparative, and synthetic study of the public health history of the United States. In the present context, however, the national material is not well integrated

with the local. Some of it is oversimplified. And there is no little repetition from one chapter to another.

Galishoff's work reads easily, and it is both original and important. One wishes, however, that it had been further pruned, more tightly organized, and more fully thought through in certain respects.

JAMES H. CASSEDY
National Library of Medicine

DAVID F. LABAREE. *The Making of an American High School: The Credentials Market and the Central High School of Philadelphia, 1838–1939*. New Haven: Yale University Press. 1988. Pp. xiv, 208. \$30.00.

In recent years historians of American education have tended to focus on schooling for the urban and rural masses, that is, those in the primary grades. Secondary schools have been given little attention, although David F. Labaree and a few others are now attempting to right the balance. In analyzing the first one hundred years of Central High School (for boys) in Philadelphia, Labaree traces the shifting meaning of secondary education for the middle class, particularly the influence of politics and the economic marketplace. He is strongly influenced by two recent trends: neo-Marxism and cliometrics.

Central High School emerged in the mid-nineteenth century with conflicting purposes in attempting to combine republican principles in the curriculum with a meritocratic thrust, thereby emphasizing individualism and competition. The school "was founded not only as an expression of bourgeois ideology but also as a mechanism for marketing the common schools to the middle class" (p. 30). And it was successful in making its diploma most difficult to achieve and most desirable for the sons of the proprietary middle class, who used it as a marketable commodity. Central High had lost its preeminent position in Philadelphia's educational structure by the early twentieth century, however, because of the rise of a political-educational bureaucracy and the emergence of other secondary schools. Still, by the late 1930s, the school had regained its former academic status and "reaffirmed publicly its familiar role as the provider of meritocratic instruction and status advantage to its long-standing middle-class constituency" (p. 92). In the process the faculty had experienced a decline in status and power within the broader educational framework yet was all-powerful in the classrooms. The curriculum, with various twists and turns, became more narrowly academic, increasingly preparing middle-class boys for college rather than for the world of work.

This study is cogently argued and adequately documented. Labaree has used quantification to good advantage, underscoring various points, for example, the class backgrounds of school graduates from 1850 to 1920, with elaborate charts and tables. He is not particularly interested in telling a story, however, but in arguing a point: "Whereas the logic of democratic governance had led to the expansion of political equality, the logic of capitalist markets has led to the growth of economic inequality" (p. 174). The history of Central High School serves to analyze the shifting nature of the marketplace and the growing dependence of the middle class on educational certification for their sons' mobility and security. Other high schools emerged in Philadelphia, at first challenging Central High's importance, but they soon took on other functions. Labaree has little to say about them, particularly the elitist Girls High School, although a contrast would surely help focus his argument. Labaree's emphasis on class unfortunately leads him away from much discussion of gender issues. And race plays no part in his story. His argument should be applicable to understanding the rise of secondary schooling in other cities, but this is problematic because of the unique nature of Central High. Yet his interweaving of neo-Marxist theory and hard data offers much to consider in understanding formal education and the development of modern society.

RONALD D. COHEN
Indiana University Northwest

KAREN HALTTUNEN. *Confidence Men and Painted Women: A Study of Middle-Class Culture in America, 1830–1870*. (Yale Historical Publications, Miscellany, number 129.) Paperback edition. New Haven: Yale University Press. 1986. Pp. xviii, 262. \$10.95.

This paperback reprint of *Confidence Men and Painted Women* (1982) is a welcome and timely publishing event that makes widely available a particularly rich and original study of mid-nineteenth-century American culture. Karen Halttunen's interesting thesis is that conventional Victorian traits—sincerity, propriety, self-reliance—were, to a large degree, defensive responses by America's middle class to a suddenly fluid social environment in which the confidence man and the painted woman were duplicitous stereotypes. Halttunen shows that middle-class Americans, threatened by the moral rootlessness and rampant theatricality of a society undergoing rapid urbanization, concocted various social rituals that were meant to shore up self-worth and promote social cohesiveness.

Halttunen draws on an impressive array of primary documents—advice manuals, etiquette books, fashion magazines, popular drama and fiction—to trace the emergence of middle-class myths and behavioral patterns. Between 1830 and 1850, when hypocrisy was rife in the urban marketplace, advice writers promoted sincerity as the highest ideal of sentimental middle-class culture. Women, in particular, were considered the proper arbiters of conduct, since their chiefly domestic role protected them from the impurities of the marketplace. During the 1830s and 1840s, Halttunen argues, the cult of sincerity manifested itself in a movement toward demure self-effacement in dress, quiet gentility in manners, and controlled grief in mourning.

In all areas, however, there was what Halttunen describes as a vicious circle in sentimental form and feeling: the widespread advocacy of sincerity created a new self-consciousness and led, ironically enough, to playacting and insincerity. As sentimental culture began to partake of the histrionic, the middle classes became increasingly taken with gaudy fashions, melodramatic parlor theatricals, and elaborate funerals. The transition from the sentimental to the theatrical during the 1850s, Halttunen adds, paved the way for the outright idealization of superficial charm by later prophets of success such as Horatio Alger and Dale Carnegie.

An especially attractive feature of Halttunen's argument is that it manages to place seemingly contradictory elements of antebellum American culture—sincerity and showmanship, trust and mistrust—in a vibrant dialectical center. Some might wish that Halttunen had said more about representations of confidence men in literature, such as Herman Melville's *The Confidence-Man*; in humor, such as Johnson Jones Hooper's *Adventures of Captain Simon Suggs*; and in urban exposés, such as George Lippard's *The Quaker City*. Also the age's master showman, Phineas T. Barnum, is assigned a surprisingly small role in this study of cultural theatricality.

But, instead of faulting Halttunen for areas she has missed, we should congratulate her for the areas she has opened up. For example, popular advice manuals have never received such careful treatment as they do in Halttunen's hands. Her tireless excavation in ephemeral antebellum literature has yielded a provocative argument that should not be ignored by historians of nineteenth-century America.

DAVID S. REYNOLDS
*Rutgers University,
Camden*

CHARLES HOFFMANN and TESS HOFFMANN. *North by South: The Two Lives of Richard James Arnold*. Athens: University of Georgia Press. 1988. Pp. xxii, 318. \$29.95.

This book is partly psychobiography, partly social history, regional study, and comparative analysis. Born in Rhode Island into a slavetrading mercantile and entrepreneurial world, Richard James Arnold was a model of a New England businessman. By his marriage in 1823 to Louisa Gindrat, he became owner of a Georgia plantation and its slaves. But what intrigues Tess Hoffmann and Charles Hoffmann is Arnold's decision to become a southern planter while retaining his Rhode Island citizenship and pursuing an active business career in the North in urban real estate and western land, shipping, and stock investments. They conclude that his "accommodation" to slavery "as part business and property and part paternalistic society" was inevitably fraught with tension (pp. x–xi). Still, that accommodation succeeded. Each year Arnold spent six months in Providence and Newport, six on his Bryan County plantations. By enlarging his initial White Hall estate through strategic purchases, he became a premier rice and cotton grower in Georgia's low country and, by carefully attending to business in the North, Arnold converted his own \$100,000 inheritance into an equivalent inheritance for each of his seven children.

On the surface Arnold's life makes an unexceptional story—the southern planter and the northern businessman can each be endlessly replicated in the history and literature of antebellum America. But the Hoffmanns sense the special drama and tension, the potential comparative insights, the possibilities of two such lives in one individual. The story of Arnold is thus a gold mine of opportunity. It is also a minefield of potential problems. In a sense, Arnold recognized that, for he carefully balanced his southern social values against his Yankee compulsions, his slaveholding paternalism against the various antislavery commitments of northern kin and friends, his rural ideal against his urban pragmatism. By adroit maneuvering he kept his southern life—and his plantations—largely intact and expanding until his death in 1873, even though during the Civil War he remained a Unionist, quasi-exiled in Providence.

Although the Hoffmanns draw on a plantation journal and on numerous other family and business papers, they depict a plantation life largely distilled from secondary works. Descriptions of White Hall, Cherry Hill, and other Arnold farms remain static and undeveloped, and discussion of Arnold's northern business life never rises above

summary description. Furthermore, the Hoffmanns barely address important questions of comparison. What does his Georgia life add to our understanding of the Yankee Arnold; or his Providence life to our understanding of Arnold the plantation owner, the adopted son of the South? Was Arnold merely an exploitative Yankee or did his values change? The comparisons made between Arnold and southern-born planters, Arnold and northern colleagues, or Arnold and absentee Yankee planters rarely go beneath the surface. Clearly Arnold's own mixture of North and South, the dichotomy on which the Hoffmanns focus, was unstable. But were its ambiguities personal, ideological, political, or social? Were its tensions and rifts the result of economics, war, or cultural disjunction?

One would like to have known Arnold, to ponder what made him tick—and survive—in his dichotomous world. I wish the Hoffmanns had speculated more daringly.

WILLIAM H. PEASE
Charleston, South Carolina

LAURENCE SENELICK. *The Age and Stage of George L. Fox, 1825–1877*. Hanover, N.H.: University Press of New England, for Tufts University, Medford, Mass. 1988. Pp. xvii, 286. \$30.00.

This book provides a lively account of the rise and fall of George L. Fox, renowned in his own time as America's funniest performer but then largely forgotten. Born in Boston and removed to New York City as a young man, Fox's element was pantomime, and he rose to the pinnacle of success in that form in the years directly after the Civil War, especially in his prodigiously performed role as Humpty Dumpty. This study of Fox provides careful, detailed illumination of an important part of American theater and entertainment. Placed in that setting, it deserves much applause.

Quite properly, Laurence Senelick assays more than a depiction of the particulars of the mute antics of pantomime personae—Clown, Columbine, Harlequin, and Pantaloon. In two general ways, the author seeks to relate Fox and his ambience to America's society of that era, and here the success is not so complete. First, Senelick contends that Fox and his family were "largely responsible for giving stage performances a good name and enticing a new audience to them" (p. xvi). Individual actors have done much to lessen our culture's suspicion of theater, and some members of Fox's family served in that way. But what the author calls "Bowery coarseness" (p. 168) stayed with Fox almost to the end of his life on the stage. Late in his career he made the move from

East Side to West Side, and, very near the end, he made an extensive tour through much of the nation. When he died in 1877, however, the United States remained predominantly a land of farms and villages and of comparatively restrained ways. If someone was to relieve the general anxiety about theater, that person would have had to be someone more in the mold of Joseph Jefferson than of George L. Fox.

Another theme—and one that more consistently informs the book—examines the "curious phenomenon" that, Senelick contends, this actor "seemed, invariably, to be at the crux of whatever major changes were happening in American life" (p. xv). This strikes me as a sound point, although it becomes less curious when it is recalled that topicality figures crucially in much comedy, pantomime or otherwise. There may well be considerable merit in the author's final, related generalization that Humpty Dumpty "distilled into one striking figure both the disarming innocence and the violent impetuosity of nineteenth-century America" (p. 223). The historian, however, may well become impatient at seeing some of the ways whereby that nineteenth-century mood and reality are established. Some of the secondary historical scholarship is sound, but an occasional reliance on, for example, a book published in 1936 by William E. Woodward or a book published in 1926 by Don Seitz leaves something to be desired.

As biography and as history of theater, this work has much to commend it. As an effort to extrapolate from Fox and his stage high jinks, it has less. Even so, it figures as a valuable addition to the cultural history of the middle two quarters of the nineteenth century.

LEWIS O. SAUM
University of Washington

THOMAS E. SCHOTT. *Alexander H. Stephens of Georgia: A Biography*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1988. Pp. xx, 552. \$37.50.

Alexander H. Stephens would seem freakish among today's homogenized, blow-dried politicians; even back in the antebellum era he was strange enough. Born in rural Georgia in 1813, orphaned in his early teens, he attended the University of Georgia on a scholarship and graduated in 1832 at the top of his class of thirteen. He was frail and unhealthy all his life, often weighing around eighty pounds and never topping a hundred. But his ego and arrogance were a match for any man, and several times the little gamecock pressed his luck to the limit in the passionate politics of a state drifting toward secession and

civil war. Gloom and melancholy persistently haunted this lifelong bachelor, but he prospered in law and politics. He became a "planter" with hundreds of acres of land and thirty-four slaves, but politics was his real calling. He treated his blacks humanely by the standards of the time, but he never doubted the conventional wisdom that blacks were inferior to whites.

He emerged a staunch Whig among the swirling tides of Georgia politics and served ably in Congress from 1843 to 1859. Basically conservative, he opposed the Mexican War and championed the Compromise of 1850, and, like almost all southern politicians, he consistently defended the institution of slavery. He became more of a sectionalist as tension increased in the 1850s, but in 1860 he supported Stephen A. Douglas of Illinois whose running mate was another Georgia moderate, Herschel V. Johnson. Stephens and other cooperationists tried to keep the Georgia convention of 1861 from taking the state out of the Union, but their dispirited efforts failed—by a narrow margin.

Stephens went to the new southern nation's constitutional convention at Montgomery and returned as Confederate vice president. His rigid conservatism and his personal dislike of President Jefferson Davis kept him from adopting a pragmatic, win-the-war-and-argue-later approach, and he opposed suspension of the writ of habeas corpus, conscription, impressment, and other emergency wartime policies of the central government in Richmond. In the last half of the war, Vice President Stephens generally shunned his duties in the rebel capital and spent most of his time at home in Georgia, edging closer and closer to Governor Joseph E. Brown's fanatic states' rights resistance that was undermining the unity of the Confederacy. Briefly he met his old Whig congressional colleague Abraham Lincoln in a fruitless peace conference in February 1865. It was far too late for the rebels to cut a deal, so Stephens went home to sulk, and two months later the Confederacy collapsed.

After almost five months of imprisonment, Stephens went home to find himself a popular celebrity, but he was also a relic, a speaker for the dead past. He opposed Radical Reconstruction, but his views no longer really mattered. He discovered the solace of a Protestantism he had abandoned in his youth, and even the death of his beloved brother Linton could be borne. He wrote a tedious, pedantic *Constitutional View of the Late War between the States* in two volumes, and he even served again in Congress from 1873 to 1882. Finally, he emerged as governor of Georgia in 1882, but failing health had driven him into a

serious drug dependency, and he died only 119 days into his administration.

Thomas E. Schott began this study as a graduate student, and it still bears some traces of the original dissertation. It is very long and detailed—but then Stephens was an active politician for almost half a century—and a few times the reader gets bogged down a little. Occasionally the prose is flowery or overripe, and a few pages of summary and evaluation are needed to conclude this study of a complex, contradictory individual. But these are minor problems; overall this is a solid biography, a balanced, perceptive examination of an able but flawed Georgian who struggled with forces far beyond his control.

F. N. BONEY

University of Georgia

STEPHEN V. ASH. *Middle Tennessee Society Transformed, 1860–1870: War and Peace in the Upper South*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiii, 299. \$32.50.

Stephen V. Ash insists that Middle Tennessee, a group of ten counties in the center of the state, comprised a "third South": "Broad prosperity, a large slave population, and widespread slaveholding sharply distinguished Middle Tennessee from the Southern highlands; yet the absence of King Cotton and of the economic hegemony of planters set it equally apart from the Deep South" (pp. xi–xii). But Ash's own evidence suggests that Middle Tennessee was simply a variant of the plantation areas of the lower South. Large slave plantations growing staple crops were somewhat less common in Middle Tennessee, but its social structure and ideology closely resembled those of the Deep South. Ash convincingly demonstrates that, although sharp differences in landed and slave wealth and professional status divided the region's antebellum whites along class lines, they were united ideologically, the elite and the common folk alike accepting slavery, paternalism, and the right of the elite to exercise patriarchal rule.

Middle Tennessee suffered extensive property losses throughout the Civil War from the marauding of occupying troops, guerrillas, and bandits. Those losses along with emancipation ended the region's prosperity and destroyed much of its wealth but, Ash argues, not its antebellum elite and the ideology that justified its rule. "The heartland's plutocracy in 1870, though considerably poorer in dollars, monopolized the region's wealth in about the same proportion as in 1860" (p. 228), and "the common people continued to sanction the traditional class structure, to accept patriarchal dominion and noblesse oblige, and to proffer their

fealty in return" (p. 232). The result, Ash insists in his most original, if controversial, interpretive conclusion, was a sharp ideological division between black and white society in postbellum Middle Tennessee. Poor and clinging to an outworn and backward ideology, the region's white society in 1870 was "an atavism, more rural, more agrarian, more hidebound than it had been a decade earlier" (p. 252). The region's blacks, however, moved in a new direction. They quickly jettisoned the patriarchal ideology, which they had only appeared to accept under slavery, and they used their freedom to establish their own communities and churches and to achieve social and ideological independence and separation from the white community. While the whites looked backward, tied to an outworn and antimodern outlook, the blacks, liberated from the physical and ideological constraints of slavery, looked forward, becoming increasingly modern in their outlook and perceptions even though they suffered from "legal and economic subjugation" (p. 253).

Ash carefully marshals an array of evidence from newspapers, private papers, government documents, church records, and the manuscript census to support his interpretation. But his evidence, although it is consistent with his interpretation, is not convincing because the brief time span he covers prevents him from fully investigating the economic foundations of postemancipation society. By 1870, Tennessee had been politically redeemed, but its social and economic structure, like those in other parts of the South, remained in a state of transition as blacks and whites attempted to adjust to free labor. Ash mentions the growth of tenancy but does not investigate it fully enough to assess its significance. He notes that, by 1870, blacks had become more urbanized and whites more rural, but, in fact, most of the population, black and white, were rural residents who worked the land under a variety of tenure forms and were developing a variety of economic relationships with landowners and merchants. Had he continued his investigation beyond 1870, and had he investigated some of the new economic and legal relationships that evolved in the decades after 1870, he would have discovered some of the variation that is now missing in his analysis.

If Ash's thesis remains unproved, it nevertheless offers a provocative insight into the nature of the postbellum South that deserves further investigation. Even those who reject his thesis will welcome Ash's richly documented description of Middle Tennessee's social history during the Civil War decade.

HAROLD D. WOODMAN
Purdue University

JACQUELYN DOWD HALL *et al.* *Like a Family: The Making of a Southern Cotton Mill World.* (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1987. Pp. xxiii, 468. Cloth \$34.95, paper \$12.95.

Cowinner of the Merle Curti Social History Award, this book is a superb history of work and workers' culture in southern Piedmont textile mill villages from the 1880s through the General Strike of 1934. In clear and compelling prose, the authors weave the threads of social, labor, family, business, and cultural history into a rich tapestry that reveals the human dimensions of regional economic transformations over half a century. The mill workers' own words provide the raw material for this ambitious enterprise, and the authors' insights into class and gender relations yield patterns of analysis that stress the individual and collective dignity of long-disparaged "lint-heads." Chief among these patterns is the theme of family—the affective, and in many cases actual blood and marital, ties that enabled workers to forge a group identity, one based on mutual respect and obligation, in opposition to the mill owners' and bosses' paternalistic authority.

The authors rely on two major forms of primary sources—wide-ranging interviews with more than one hundred thirty Piedmont working people and letters to Washington officials from women and men who revealed in poignant and dramatic detail the shortcomings of the National Recovery Administration (NRA) Textile Code during the first two years of the New Deal. The depression-era correspondence is especially remarkable in terms of the political voice it provided housewives and daughters; the authors suggest that "verbal expressiveness lay at the heart of the 'porch culture' through which women pieced together a neighborly, kin-oriented community life," and so the letters, "written in private, drew on the female art of conversation" (p. 309). Moreover, sending a letter to Franklin Roosevelt, Eleanor Roosevelt, Secretary of Labor Frances Perkins, or Hugh Johnson of the NRA required a great deal of courage on the part of family members who faced the prospect of eviction or starvation at the hands of vengeful blacklists.

The extraordinarily detailed information provided by these sources gives flesh-and-blood presence to workers traditionally prone to melodramatic and stereotypical depictions. Martin Lowe recalls the hand-to-mouth existence that forced him to move his family out of the Tennessee hills—"Now times was hard back there; it was the dickens" (p. 37)—to find "public work" in the mills. With excruciating precision Icy Norman tells of her ordeal as a young girl learning to tie a

weaver's knot, caught between her mother's example and the foreman's uncompromising demands. Edna Hargett sheds light on damp, stifling workrooms, where pranks (like substituting cayenne pepper for snuff) could relieve the noisy monotony and at the same time serve to initiate a "green" worker into the etiquette of life among the weavers. When, in a letter to Franklin Roosevelt, a South Carolina woman denounces mill owners as "old slimy serpents crawling spitting their Poison fighting your Program" (p. 325), she reveals her coworkers' outrage over the layoffs and stretch-outs that inspired massive union activity in the 1930s.

The book begins with a consideration of the grim life that pushed people off the countryside and into the mills. The authors proceed to examine the inevitable conflict, as well as more surprising instances of accommodation, between a rural folk culture and industrial work discipline. Sections on technological change, popular culture, religion, and labor conflict in the 1920s and 1930s are enlivened by the personal histories of five individuals from the Piedmont Heights village near Burlington, North Carolina—an owner-entrepreneur, a plant supervisor, a Baptist preacher, a woman worker with forty-seven years of mill experience, and a resident physician. The book ends with the ambiguous legacy of the General Strike of 1934, a militant uprising suppressed through violence, blacklisting, layoffs, and the replacement of workers with machines.

Significantly, the authors avoid romanticized or condescending views of workers and their values. For example, in their discussion of mill village churches, they outline the complex interaction among owners, preachers, and congregants: "the relationship between many mill churches and mills was symbiotic and reciprocal, but not one in which power was shared equally" (p. 283). Found within these pages are women, men, and children who express pride in their work, take pains to provide for their families, create and sustain new musical forms, and demonstrate more political acumen and labor militancy than their national union (the United Textile Workers of America) could ever claim.

Despite the book's many virtues, its authors at times lose sight of obvious issues and questions. For instance, black people are conspicuous by their absence from this volume on southern working-class history; the authors might have speculated on the implications of a lily-white textile force for southern society as a whole. The suggestion that mill employees perceived blacks "in terms that differed little from those of skilled workers angry at being reduced to the level of the unskilled" (pp. 317–18) reflects a growing, and

unfortunate, tendency among southern historians to conflate race and class as categories of social experience.

Finally, the theme of family, developed so forcefully in early parts of the book, fades from view by the time the climactic decade of the 1930s is explored. When the authors chronicle demographic and economic changes within communities, they imply either that divisions between "desperate newcomers" (p. 346) and union supporters amounted to a bitter, bloody family feud or that the mill "family" had indeed ceased to exist altogether. In the end, the family analogy is a rather vague and sentimental one, revealing little about the allocation of power and authority (based on age, sex, skill, or some combination of the three?) among community members themselves. "Millhands' habits and beliefs were more than remnants of a rural past; they were instruments of power and protection, survival and self-respect" (pp. 179–80). But this "distinctive mill village culture" was insufficient to shake the firm grip that lords of the loom held over the wages and working conditions, if not the souls, of millhands throughout the Piedmont.

JACQUELINE JONES
Brown University

DANIEL ROSENBERG. *New Orleans Dockworkers: Race, Labor, and Unionism, 1892–1923*. (SUNY Series in American Labor History.) Albany: State University of New York Press. 1988. Pp. x, 233. Cloth \$44.50, paper \$14.95.

Historians have long recognized that the late nineteenth and early twentieth centuries constituted the nadir of race relations in the United States. But, as Daniel Rosenberg demonstrates in this book, even in the worst of these years patterns of race relations were not monolithic. Building on earlier studies documenting the reality of limited interracial cooperation among some sections of southern labor, Rosenberg traces the development of an impressive tradition of interracial unionism and a remarkable era of union power on the New Orleans waterfront.

Waterfront employers in the Crescent City often sought to play one race off against the other and especially to use lower-paid black labor as a means of weakening the position of white workers. Thus, "in order to survive" (p. 56), white unionists began to offer cooperation to blacks. This cooperation remained more or less intact until the depression of the 1890s, when competition for jobs led to armed confrontations between whites and blacks and a worsening of the conditions of both. Drawing on the painful lessons of the 1890s, both

groups again concluded that survival required solidarity across racial lines.

The most advanced and innovative forms of cooperation developed among the screwmen, the aristocrats of the levee, whose name derived from the jackscrew they used to stow giant bales of cotton in a ship's hold. They decided not only to equalize the work among blacks and whites but also to work in integrated gangs, with two blacks and two whites assigned to each hold. In important respects, the application of this "half-and-half" principle continued to favor whites over blacks, but overall it represented a sharp challenge to the rigid segregation that was becoming normative throughout the South. Moreover, "half-and-half" was carried over from the workplace to the unions. Although organized in separate locals, whites and blacks from every waterfront craft joined together in the Dock and Cotton Council on which an equal number of delegates from each race represented eight organizations with a total of ten thousand members. When the unions flexed their muscle, as they often did, especially in the twenty-day general levee strike of 1907, white supremacists charged that the Crescent City was "practically under negro government" (p. 155).

The waterfront employers shared the prevailing hostility toward interracial unionism, but they were even more concerned about the stubborn control that the unions exercised over the pace of work on the docks. They complained that Galveston dockworkers stowed far more cargo in a day than their New Orleans counterparts, because the Texas port was "less labor union ridden" than the Crescent City (p. 144). And they bitterly attacked the screwmen, whose craft they regarded as obsolete, for their relentless opposition to increased productivity. What is remarkable is that the screwmen were able to exercise so much power for so long. They were the hub around which waterfront unionism revolved. For two decades, thousands of other dockworkers followed their example and at critical moments demonstrated the solidarity that allowed them to survive. It was not until the 1920s that the employers were able to crush the Screwmen's Benevolent Association, and when they did so the entire edifice of waterfront unionism was reduced to a shell.

Rosenberg's well-written monograph tells the story of the New Orleans dockworkers clearly and concisely. If there is room for criticism, it concerns the narrowness of the author's focus. He is correct to make race the central theme, but his preoccupation with race seems to have crowded out a fuller consideration of other major questions and a sense of the larger national context in which unionism developed. For example, he characterizes the Dock and Cotton Council as a kind of

industrial union, but its dynamic core was the screwmen, whose commitment to an obsolescent skill became the centerpiece of conflict on the docks. The fact is that interracial cooperation and broad labor solidarity coexisted with persistent craft distinctions. It was employers, not workers, who wanted to obliterate these distinctions. The Dock and Cotton Council was less an industrial union than a craft federation, a halfway house between the predominant craft unionism of the AFL and the commitment to industrial unionism that was gaining momentum on the Left during this period. The council had its counterparts in other cities and industries, just as the pattern of class conflict on the New Orleans docks was similar to that in other ports. True, race played a more central role in the Crescent City. But the struggle for control of the work process, the unions' move toward federationism, and the employers' anti-union offensive occurred in city after city, and the triumph of the open shop came at nearly the same time in many of them.

Although some readers may come away from Rosenberg's study with a sense that important avenues could have been more fully explored, perhaps they will also acknowledge that, thanks to him, we now have a much better understanding of another vital chapter in Jim Crow's strange career.

BRUCE NELSON
Dartmouth College

JOHN M. GLEN. *Highlander: No Ordinary School, 1932-1962*. Lexington: University Press of Kentucky. 1988. Pp. ix, 309. \$30.00.

The Highlander Folk School was no ordinary institution of learning. According to its critics, "it had no normal faculty, awarded no diplomas, [and] never had its courses approved by state officials" (p. 187). What detractors found most contemptible, however, proved to be the source of its greatest value. For some thirty years, a band of dedicated activists in Monteagle, Tennessee, combined progressive politics with innovative adult education in attempts to reshape southern life along democratic and egalitarian lines. They sought to empower oppressed industrial workers and impoverished farmers, white and black, to seize greater control over their lives by challenging officials who exploited and divided them. This objective, more than any failure to grant graduation certificates, aroused the ire of southern conservatives determined to maintain race and class distinctions in the most economically devastated region of the nation.

Established in 1932, the Highlander Folk School was modeled after Danish folk schools and the

community outreach programs of the YMCA. Its guiding light, Myles Horton, had a formal education, possessed a college degree, and took post-graduate courses at Union Theological Seminary under Reinhold Niebuhr. These credentials notwithstanding, Horton cast aside traditional ideas about adult education and created a curriculum that emphasized self-discovery, cooperative planning, interracial brotherhood, and the power of folk music to sustain collective action. Through residential classes and workshops at the school and extension programs that thrust Horton and his staff into the middle of tense labor and racial controversies, Highlander trained thousands of indigenous leaders who waged the struggle for social justice in their own communities. The school did not initiate the battles for unionization in the 1930s and 1940s or the desegregation efforts of the 1950s and 1960s, but it did serve as an educational auxiliary to the Congress of Industrial Organizations (CIO) and the Southern Christian Leadership Conference (SCLC). Indeed, the SCLC's voter registration campaign, Crusade for Citizenship, originated at the folk school.

From its very inception, the enterprise encountered deep hostility from southern business and political leaders, who viewed it as a subversive threat to the established order. Dubbing it a "Communist training school," they conducted investigations, spied on the school, and brought legal and extralegal pressure to shut the center. Even the CIO drifted away from the school during the post-World War II red scare. Although Highlander became a convenient target of segregationist assaults against its civil rights activities after the *Brown* decision, technical violations of state law rather than radical ideology ultimately provided the legal grounds for closing it down. Tennessee prosecutors convicted school administrators for acting improperly under the terms of the school's charter by, among other things, selling beer to participants at their sessions. The school went out of existence in 1961, only to reopen with a new charter as the Highlander Research and Education Center, which continues functioning outside of Knoxville.

John M. Glen provides a tightly written and carefully researched account of the school's years. Covering both major and minor episodes in the organization's life, his judicious account presents a sound appraisal of the accomplishments of the school without exaggerating its contributions, while appropriately acknowledging its shortcomings. Yet one wishes that Glen had captured more imaginatively the excitement this unconventional institution generated, but his orthodox, chronological approach as well as his focus on administrative details prevent him from doing so. Al-

though he also intended this book to serve as a biography of Horton, he does not portray very vividly the personality of the man who, for over three decades, kept the school running. Nevertheless, this solid study of a remarkable experiment in social change has special interest for historians of the South, labor, and civil rights.

STEVEN F. LAWSON

University of South Florida

RODMAN W. PAUL. *The Far West and the Great Plains in Transition, 1859-1900*. (The New American Nation Series.) New York: Harper and Row. 1988. Pp. xv, 381. \$24.95.

Rodman W. Paul finished this book just before he died, and his colleague Martin Ridge generously saw it through to press. To review such final words of a loved and distinguished scholar poses problems, but I believe I am objective when I conclude that this book is a splendid survey of western American history, which in the New American Nation Series will stand graciously between the volume by Ray Billington, *The Far Western Frontier, 1830-60* (1956), and the forthcoming work by Earl Pomeroy on the West in the twentieth century.

The book begins with Horace Greeley setting the stage through his western travels of 1859, and it concludes with compilers of guidebooks in 1900, such as the writer for Baedeker, describing change so rapid that its capture "becomes almost futile" (p. 295). Between the two, Paul stresses that rapidity of change, especially as the pioneer phase of settlement gave way to a capitalized, industrialized economy. He emphasizes mobility and census data, the growth of urban populations, and, in general, economic development (Salt Lake City's merchants get almost as much attention as do its religious figures). A principal theme is the driving force of individual gain triumphing over the sense of community. He ends his chapter on the cattle industry, for example, on a negative note because of the typical rancher's lack of public spirit. Thus, Paul finds a moral: "learning to cope with the western environment . . . was only a part of the task. The rest was the product of our litigious nature and our carelessness in using natural resources and in staffing the legislatures and courts that pass judgment upon our practices" (p. 251).

Two chapters are unusual in broad histories of the West. One discusses sheep ranching, which Paul claims was as significant as the more familiar cattle kingdoms. The other is a chapter on later mining in which the smelting and refining of base metals by trained engineers and geologists over-

shadowed in economic significance the simpler days of gold and silver.

Paul focuses heavily on mining, as might be expected from a man who spent his life on the subject. This emphasis appears not only in the mining chapters but also with other subjects, such as transportation and agriculture, that are organized, probably correctly, as responses to the mining frontier. Although there are excellent sections on Mormon polygamy and ethnic groups (the Spanish-speaking New Mexicans and the Chinese are especially well handled), modern social history in the sense of inclusion of women and the family is not much attempted.

Inevitably, historians with their own wish lists will find something missing. Some will want more attention to the environment (John Muir is not here); middle westerners will want more Kansas and Nebraska and less California; water historians will applaud the discussions of water usage and law but will miss the mention of George Chaffey; intellectual historians will seek more interpretation, more attention to religion and education, more thinkers like Henry George.

The book, however, takes a broad view of many western themes. Racism in the West is seen as part of a national deficiency. The Mormon shift from communitarianism to individualism is given a national context. Violence is compared to violence elsewhere. Western mobility is shown to be not that different from American mobility. The West is clearly a part of the American nation.

Paul uses a quotation from Oliver Wendell Holmes to title his chapter on western cities (Denver, Salt Lake City, Portland): "Build thee more stately mansions." This book is indeed a stately mansion to complete a lifetime of work in western American history.

ROBERT V. HINE
University of California,
Riverside

NATHAN W. PEARSON, JR. *Goin' to Kansas City*. (Music in American Life.) Urbana: University of Illinois Press. 1987. Pp. xviii, 252. \$24.95.

By the mid-1920s, Kansas City had become the creative center of jazz in the Southwest, the territory, according to local perceptions, stretching west from Missouri to Colorado and north from Texas toward the upper Midwest. The city's musical gifts survive on a bounty of early recordings; its musical life has been recounted in interviews, criticism, and a book, Ross Russell's *Jazz Style in Kansas City and the Southwest* (1971). Yet music historians are still left guessing about many details that have never extended beyond the musicians'

inner circle. In this study, Nathan W. Pearson, Jr., taps that rich source, shaping a work based on the recollections of the musicians themselves.

Pearson's book began as an oral history project for a touring exhibit launched at the Kansas City museum in 1980. Through the help of local patrons, he and another historian, Howard Litwak, located and interviewed several early Southwest musicians, many of whom had left music years ago. Their recollections form the basis of Pearson's eighteen-chapter study. Arranged chronologically and thematically, the book first charts the histories of some important early touring ("territory") bands and band leaders. It then explores the emergence of Kansas City jazz, also providing some excellent firsthand accounts of the mob-run entertainment business that supported popular music making.

The book's foremost contribution is the quality of its oral research. Pearson provides some impressive detail on several important musicians—including George E. Lee, Torrence "T." Holder, and Jesse Stone—about whom and from whom we have previously heard little. The narratives offer a sense of the life of the Southwest jazz player and that player's ambitions, aims, struggles, and successes. Thematic arrangement of the musicians' accounts makes for a readable text and increases the book's usefulness as a research tool. Pearson, moreover, works to clarify many of the musicians' casual references. At times, however, and particularly in the early chapters, the reader is left wondering about the order of events, something that the accompanying chronology (appendix A) could have helped clarify. The book also would have benefited from a discography of original 78-RPM recordings, a common item in recent scholarly studies of jazz.

Missing above all from the book is an analytical framework placing the musicians' statements in historical perspective; we get the facts but little sense of their implications. When Pearson does provide interpretation, he usually relies on secondary works that we have read all too often. This reliance on secondary information also carries over into his musical critiques. Pearson tends to shy away from addressing the importance of Kansas City jazz in musical terms, preferring to paraphrase standard textbook writing. The weakness shows up particularly in a chapter titled "The Kansas City Style," an all-too-brief account of performance practice. No doubt Pearson's foremost aim was to document an important period in jazz history and to offer some sense of the poetry of the musicians' lives. He has accomplished that task well. But he would have greatly enriched his book by encouraging the musicians to speak deliberately about their art or, failing that, providing

some of his own insights into their aesthetic aims and accomplishments. Such a perspective would have set the book above the standard. It would have also made it more musical.

RONALD M. RADANO
*University of Wisconsin,
Madison*

IRA G. CLARK. *Water in New Mexico: A History of Its Management and Use*. Albuquerque: University of New Mexico Press. 1987. Pp. xviii, 839. \$50.00.

This large book is a landmark study. Ira G. Clark has organized it well and written it carefully, and he exhibits balanced judgment. Two fine chapters deal with the aboriginal and Spanish-Mexican epochs; eight cover the period from 1848 to 1910; and fourteen bring the story into the early 1980s. Clark has made exhaustive use of federal and state court records and administrative publications, scientific studies, newspapers, scholarly literature from many fields, and unpublished papers of the New Mexico State Engineer Office.

Water policies in New Mexico have shifted with the economy, from "subsistent farming-grazing through range cattle to irrigated agriculture and finally toward multiple use" (p. 695). Unchanging has been New Mexico's influential doctrine that water is state property, "subject to appropriation for beneficial use, with beneficial use the basis, the measure, and the limit of the right" (p. 368). Competition, urgency, and complexity of activities have steadily increased. Rising population and additional industries have caused stress, and the state has had to use all available water resources. Because most groundwater is brackish or saline and almost all reasonable dam sites are in use, great effort has had to go toward restoring the quality of water and toward conservation.

New Mexico is one of the arid states where the most worthwhile product of national forests is water. Clark shows in detail how new laws, uses, and demands have been integrated with existing policies and activities. The book is a manual of economic shifts and of agencies, programs, and laws and their various applications, traced carefully from Washington, D.C., or the state capital to the local communities. It is a history of cooperation among federal, state, and local public agencies and private groups, but it also reveals when conflicts have blocked solutions—for instance, the poor record at all governmental levels of award of water rights to Indians. A constant theme is the importance of the state engineer in surveys, allocation of water rights, groundwater and surface water regulation, flood and pollution control, regulation of industrial use, interstate negotiations,

domestic use, and basin regulation. I could not find the answer to two questions: how much is being done to inspect old dams and how the duty of water (how much one may have per acre in irrigation) may be differentially applied. The constant pressure exerted by citizens and governments has been for most use soonest; there has been less concern for restoring water quality and least for preparing for future needs. Clark believes that a deteriorating situation calls for choosing the "least worst" among alternatives (p. 695). Of those now available—seeking new supplies, stretching supplies through better management, and curtailing demand through conservation and better use—Clark favors the third, including education of residents on more effective use of resources. This fine study makes it clear that demand keeps outrunning knowledge and that today's water management system does far too little to find the facts or do the planning that we will so badly need tomorrow.

CLIFTON B. KROEBER
Occidental College

JOSEPH E. STEVENS. *Hoover Dam: An American Adventure*. Norman: University of Oklahoma Press. 1988. Pp. ix, 326. \$24.95.

In recent years a growing number of historians have decried American efforts to subdue nature through technological means in view of increasing ecological problems. As Joseph E. Stevens notes, they have argued that twentieth-century technology "is untrustworthy, exploitative, and destructive of the environment, and the human spirit" (p. 267). To some extent, such critics have read the concerns of their own age, the 1980s, into past American experience. But technology in other periods of the nation's history was not always necessarily destructive, nor was the prevailing mood like that of some academicians in the 1980s. This volume details the story of the building of Hoover Dam, one of the technological marvels of twentieth-century America, and seeks to provide an antidote to contemporary pessimism by underscoring the technological accomplishment that it represented when it was completed in the 1930s. As Stevens notes in his elegantly written and colorful volume, "in the clear desert light of Black Canyon, guilt about the deeds of the past and doubts about the promise of the future shrivel. The romance of the engineer still lives in the graceful lines and brute strength of Hoover Dam" (p. 267). Within the grand theme of man against nature in the West, Stevens provides a powerful narrative detailing the struggles of men, women, and machines to subdue the mighty Colorado

River. Although other historians have delved into particular aspects of Hoover Dam's construction, Stevens's book is, curiously enough, the first full account of the gigantic project. Stevens tells the fascinating story of this "great pyramid of the American West, fount for a twentieth century oasis civilization" (p. vii), from its beginnings in 1931 to its completion five years later. In seven fast-paced chapters, he details the initial dreams about harnessing of the river and the extraordinary difficulties involved in building what at the time was the world's largest structure. Stevens has a flair for dramatizing the human interest of the enterprise and keeps his focus on people involved in the venture. Some, like Henry Kaiser, became famous. But Stevens devotes particular detail to the everyday lives of the vast army of men and women who came to labor on the dam—many of them unemployed victims of the depression, single men as well as families, who streamed into the remote desert outpost near Las Vegas.

The book provides more than narrative, however, for the analysis of the project also serves as an antidote to the historians who have decried federal water policies in the West during the twentieth century. In assessing the impact of Hoover Dam, Stevens notes that it turned the arid Southwest into "America's new technological and agricultural promised land, ushering in an era of material wealth and physical comfort" (p. 258). The long-term impact was considerable, apart from boosting the economy of Nevada. The dam changed the way westerners thought about their region and its future. It inspired a new optimism that helped break the East's stranglehold on the West as an economic colony. It also inspired President Franklin D. Roosevelt to include centralized national resource planning in his New Deal program. And it must not be forgotten that Hoover Dam provided much of the essential power needed for achievement of the production miracle of World War II.

The author's research is exhaustive and impeccable. He combed most of the relevant documents in national and local archives and court records, conducted scores of interviews, read through a mountain of newspapers and magazines, and made good use of available secondary works. Especially for a first book, this is a fine achievement, and the volume deserves to attract a wide circle of readers.

GERALD D. NASH
University of New Mexico

ROBERT E. FICKEN. *The Forested Land: A History of Lumbering in Western Washington*. Seattle: Univer-

sity of Washington Press, for the Forest History Society, Durham, N.C. 1987. Pp. xv, 324. \$25.00.

Amid the ever-popular—and seemingly ever-growing—assemblage of lumberjack tales, anecdotal histories, and coffee-table pictorials, a body of sound scholarship about lumbering in the Pacific Northwest is gradually appearing. Robert E. Ficken's book, the first survey of lumbering in western Washington, takes its place among the important works examining the state's most fundamental natural-resource industry.

Ficken summarizes the early history of western Washington with new insight. Stories such as those of the Astor party and the Hudson's Bay Company are well known; Ficken provides a new twist by detailing the early spar trade lumbering of the Astor party and outlining the significance of Fort Vancouver's sawmill to the economic plans of the Hudson's Bay Company.

Large-scale lumbering began in earnest after the discovery of gold in California, and for over three decades Washington sawmills prospered or stagnated according to the needs of San Francisco-owned mill companies. The arrival of transcontinental railroads brought dramatic change to the industry. With the depletion of forests in the Great Lakes, wealthy lumbermen looked westward. Led by Frederick Weyerhaeuser, these timber barons purchased immense amounts of stumpage in the state at the turn of the century and eventually began manufacturing at some of the world's largest sawmills.

Lumbering in western Washington in the first four decades of the twentieth century was largely the story of companies established during this Great Lakes invasion adapting to changing labor and environmental demands. Ficken ends his book with the 1930s, claiming that the rise of aerospace, electronic, and chemical industries during World War II "eroded the traditional importance of forest exploitation" (p. xiv).

This meticulously researched book will become a standard reference. It is well-indexed and contains a valuable bibliography. It is not, however, without a few faults. Those seeking a social history of lumbering in western Washington must still wait. Although Ficken criticizes anecdotal reminiscences, he ignores the human drama of lumbering—the impact of logging and sawmilling on the men and women who lived and worked in the woods and the mill towns. For example, he makes no use of the numerous and invaluable oral history resources that have been gathered from "common folks"—flunkies, mill workers, lumberjacks—at local historical societies throughout the Northwest, and his account thereby suffers.

Ficken makes a strong case for studying western

Washington alone, pointing out essential differences between lumbering there and in Oregon, British Columbia, and the forested areas east of the Cascades. But his confined vision at times leads to too narrow a view. For example, his examinations of cooperative fire control and the Wobbly strike of 1917 would have been enhanced by a further discussion of those movements as beginning east of the Cascades and moving west, just two examples of the long and important interconnection between the two lumbering regions.

And, although a book must eventually end, it seems artificial to stop this one with World War II. Washington's industrial base diversified with that conflict, but lumbering remains a key economic activity.

A few minor flaws—the fault of the publisher, not the author—are irritating. The dust jacket's biographical sketch claims Ficken is now writing a bicentennial history of Washington, something that will come as a surprise to the state's centennial commission, which hired him. The index to illustrations refers twice to the Pole and Talbot mill, which should be Pope and Talbot. And the book's too-brief, eight-page photographic section is unimaginative and unappealing, with credit information awkwardly printed upside down.

In fairness, no single volume could synthesize the entire story of Washington lumbering. But Ficken has done an admirable job of providing an economic and political survey. His book will remain an essential resource for Pacific Northwest scholars for many years.

KEITH C. PETERSEN
Pullman, Washington

MICHAEL L. SMITH. *Pacific Visions: California Scientists and the Environment, 1850–1915*. New Haven: Yale University Press. 1987. Pp. ix, 243. \$26.50.

This tightly argued book suggests a temporary California exceptionalism: in the late nineteenth century, earth and life scientists in the San Francisco Bay Area constituted an intellectual and political force quite different from that of their scientific colleagues back East. Michael L. Smith examines the careers of John Muir, David Starr Jordan, Clarence King, Josiah Whitney, George Davidson, Alice Eastwood, Joseph Le Conte, and others who worked or lived in California from 1850 to 1915. The Californians resisted the specializing, privatizing, professionalizing trends that were overtaking eastern scientists and instead urged a more generalist, ecological viewpoint that anticipated modern environmentalism. By World War I, though, the Californians had been assimilated into national trends.

According to Smith, the Californians were different essentially because their terrain was different. The state offered a stupefying variety of climate and geography, of land and life forms. It thus encouraged adaptations by its flora and fauna to a unique environment rather than rapacious competition among predators. Hence, the California scientists tended to embrace Darwinism without the Malthusian edges, what Smith calls “a kind of social Humboldtism” (p. 136). (Muir, Whitney, and King were especially impressed by Alexander von Humboldt.) The Californians maintained public roles and outside fieldwork instead of retiring to their arcane laboratories. They mingled scientific and aesthetic interests even while the two cultures were diverging elsewhere.

Of the biographies presented by Smith, his treatment of King offers the most surprises. As one might expect of a man who kept a secret wife and children, King appears quite different in his private journals than in his published writings. The mountaineering exploits described as routine in his field notes became daringly difficult when King wrote for a larger audience. In 1864 he noticed glacial evidence in Yosemite, but on the record he dutifully agreed with Whitney, his boss, and scorned Muir's theory of the valley's glacial origins. Softer and more sympathetic in private, King struck virile postures in public. Smith effectively shows how similar genderizations overtook most of the California scientists, to the ultimate loss of their discipline.

This book, thoughtful and generally well researched, is vulnerable on two points. Some of the manuscript research seems thin. Muir is the book's central figure, and the Hetch Hetchy episode its climactic event. Yet Smith's use of the Muir papers was—to judge from the footnotes—essentially limited to Muir's manuscript autobiography and to correspondence from the late 1890s. And Smith did not exploit the shelves of Hetch Hetchy files in Record Group 79 of the National Archives, a basic source for any student of that controversy.

Second, Smith follows Samuel P. Hays, in Hays's *Conservation and the Gospel of Efficiency* (1959), and declares that American conservation began among scientists. As John F. Reiger and other recent scholars have shown, conservation pioneers also included many amateurs: birders, mountaineers, hunters, fishermen, and members of women's clubs and civic groups. In the East the professionally trained, scientifically aspiring Gifford Pinchot was less significant in the 1890s than George Bird Grinnell, a zealous sportsman, or Charles Sprague Sargent, the Boston tree lover. Smith points to a heavy representation of scientists on the Sierra Club's board of directors from 1892 to 1914. But the directors were mostly show horses, trotted out

to prove the club's seriousness and connections, while the group was really sustained by such amateur workhorses as Will Colby, Marion Randall Parsons, and William F. Badè. Conservation grew up as a noisy dialogue between amateurs and professionals, each faction needing the other to fill in its deficiencies.

Those cavils aside, Smith's book is significant for historians of science, of California, and of the American environment.

STEPHEN FOX
Boston, Massachusetts

PETER J. KUZNICK. *Beyond the Laboratory: Scientists as Political Activists in 1930s America*. Chicago: University of Chicago Press. 1987. Pp. x, 363. \$29.95.

Scientists have seemed to have been curiously exempt from the progressive and leftist political activism that suffused many social groups during the 1930s. That apparent exception no longer holds. In a well-researched, provocative book, Peter J. Kuznick demonstrates that many scientists became involved in the decade's liberal to leftist political causes.

Scientists were slower than most intellectuals to arrive at that position. Tied to corporate funding and social conservatism in the 1920s, most scientists remained conservative in the early 1930s. But international events jarred scientists into a reassessment of their role in society. The Nazis' perversion of science and assault on scientists highlighted scientists' vulnerability to political manipulation. Conversely, the Soviet Union, which seemed to direct science toward socially beneficial ends, offered an alternate model through the middle years of the decade. Young British Marxists, such as J. D. Bernal, proved influential in the transmission of the doctrine of bending science toward social goals.

By 1937 the American Association for the Advancement of Science regularly included sessions on science and society at its annual meetings, and in 1938 Walter Cannon, a socialist physiologist, was elected president of the organization. The aging anthropologist Franz Boas mobilized scientists against Nazi racist doctrines and in defense of intellectual freedom, a development that culminated in the formation of the American Committee for Democracy and Intellectual Freedom. And in late 1937 the American Association for Scientific Workers (AASW) was formed to provide a platform for progressive scientists. Believing that social concern became the norm for scientists, Kuznick writes: "A remarkable transformation occurred in the inner world of scientists—the beliefs, attitudes, and assumptions that defined their so-

cial identity and sense of purpose—a change far more profound than that experienced by any other sector of the American population" (p. 253). That conclusion is also a statement on how far scientists had to travel in comparison to other groups.

Kuznick has documented an important and neglected aspect of the social history of science. By demonstrating the roots of scientists' political activism, he gives new meaning to subsequent developments such as the scientists' movement of 1945–47 and scientists' environmental and antinuclear efforts. The book also draws science into the history of American radicalism and illuminates a vital aspect of the intellectual response to the economic and international crises of the 1930s. Yet Kuznick's conclusion should not obscure the perdurance of centrist to conservative attitudes in the scientific establishment. He notes that the AASW was vitiated by red-baiting. Although Kuznick does not provide a systematic analysis by discipline or age, most scientific activists appear to have been relatively young and to have worked in the life sciences and mathematics, not physics and chemistry. A key role was assumed by Boas, whose discipline lay outside the scientific mainstream. Daniel J. Kevles has observed that American senior scientists were "generally conservative" (*The Physicists* [1978], p. 288). Moreover, as World War II and the cold war displaced the international crises of the 1930s, most scientists decided that their social responsibility lay in bolstering the state. Kuznick's redemption of a vital moment in the history of American scientists should not obscure the necessity of understanding the boundaries—institutional, financial, and ideological—to leftward movement among them.

CLAYTON R. KOPPEL
Oberlin College

CHARLES E. ROSENBERG. *The Care of Strangers: The Rise of America's Hospital System*. New York: Basic Books. 1987. Pp. x, 437. \$22.95.

Charles E. Rosenberg's study makes a major contribution to the historiography of hospitals in America. Until recently historical studies of hospitals in America were typically institutional accounts of individual hospitals. In the last decade or so a new generation of medical historians, immersed in social history and interested in the themes of class, ethnicity, and gender, has examined individual hospitals within the larger context of American society. Rosenberg moves beyond the focus on individual hospitals. He defines the hospital as a central institution in the shaping of American society in the same way that other

historians have interpreted the asylum and the school. In doing so, he provides a model for understanding the evolution of the hospital as a major institution in American culture.

Rosenberg argues that the character of the hospital with which one is familiar in the late twentieth century was in place by the 1920s. The emergence of the modern hospital occurred over a half-century spanning the decades from the 1870s through the 1910s. Throughout most of the nineteenth century, the home was the preferred site of medical care. The hospital was an institution of charity that served the medical and social needs of the urban poor. Only those without access to home care went to hospitals, seeking food, shelter, and rest, much as they sought medical treatment. At the same time, elite physicians cared for hospital patients without charge, out of a tradition of social obligation as well as professional self-interest. Meanwhile, trustees ran hospitals because of their commitment to benevolence and their concern for social control in a rapidly urbanizing and industrializing society. As an institution built on the prevailing emphasis on social responsibility and personal contact, the hospital reflected the close connection between medicine and morality in nineteenth-century America.

In the last decades of the nineteenth century and the early decades of the twentieth, changes in medicine and in society at large combined to reshape the hospital in American society. The discovery of the germ theory of disease, the development of laboratory science, and the promise of safe, aseptic surgery helped transform the hospital into an institution where medical science superseded earlier social concerns. The growth of modern science coincided with significant changes in the medical profession, such as the restructuring of medical education and the consolidation of power within the regular wing of the profession. The hospital became the cornerstone of modern medical education. As formal clinical training became a necessary component of medical training, medical students and young doctors turned to the hospital for a substantial part of their medical education. At the same time, established doctors gradually sent their private patients to the hospital to benefit from the facilities of modern medical science. The result of all of this change was that the medical profession became "hospitalized," as Rosenberg astutely observes, precisely as the hospital became medicalized.

Coinciding with these changes in the medical profession were parallel changes in nursing practice. Rosenberg shows that the origins of nursing were inextricably linked to the development of the hospital. In hospital training schools for nurses, women received formal training and ultimately

replaced the nurses of an earlier era, that is, recuperating patients and well-meaning volunteers. The new nurses not only provided the hospital with a stable, disciplined, and inexpensive supply of workers but also contributed to the efficiency, professionalism, and scientific culture of the emerging hospital.

All of these changes coincided with larger changes in American society as the moralism of Victorian-American culture began to give way to the objectivity, empiricism, and so-called value-free ideals of a rising culture of science. By 1920, the hospital, once a haven for the sickly poor, had evolved into an institution of modern medical science. The hospital replaced the home as the locus of medical care and emerged as the place where the most advanced medical technologies and therapies were available. At the same time, the hospital released its tight hold on its responsibility for the social well-being of its patients. As a result, doctors took over much of the decision-making responsibilities that lay trustees had exercised in an earlier era. As Rosenberg explains, the fundamental shape of the hospital as it emerged in the 1920s already embodied the very strengths and weaknesses of the hospital as it now stands at the end of the twentieth century.

This study is an elegantly written book that broadens the history of hospitals and places it squarely within the larger field of American social history. In doing so, the book makes a major contribution not only to the history of medicine but also to the history of institutions and to American social history in general.

VIRGINIA G. DRACHMAN
Tufts University

MICHAEL P. MCCARTHY. *Typhoid and the Politics of Public Health in Nineteenth-Century Philadelphia*. (Memoirs of the American Philosophical Society, number 179.) Philadelphia: American Philosophical Society. 1987. Pp. ix, 101. \$12.00.

Michael P. McCarthy confronts a chapter in Lincoln Steffens's story of dirty politics (*The Shame of the Cities* [1904]) in this play-by-play account of the cleansing of Philadelphia's water supply. With the growth of population in Philadelphia and its surroundings, the need for public services became the source of conflicts fueled, in the postbellum decades, by factions among Pennsylvania Republicans and Democrats and inadequate municipal resources. Steffens accused Philadelphians of being "corrupt and contented," and more recently Sam Bass Warner, Jr., has suggested that failure to control pollution of the rivers supplying Philadelphia was the consequence of favoring private over

public interests (*The Private City: Philadelphia in Three Periods of Its Growth* [1968]). A death rate from typhoid fever of 99 per 100,000 citizens in 1899 ranked Philadelphia among the sickliest cities in the nation (Pittsburgh topped the list at 111 per 100,000).

McCarthy shows that Philadelphia's inability to supply clean water in amounts adequate for public sanitation and personal hygiene provided the stage for airing deep running political and scientific controversies. His close analysis of Philadelphia's party politics, the water supply available from the murky waters of the Delaware and the Schuylkill rivers, and the scientific knowledge and social resources available at the turn of the century leads him to conclude that the city administrators were already making an honest effort to meet the crisis in the 1880s. By the end of the century, when political wisdom and engineering science appeared to open the way for expert advice on the best methods to increase and purify the water supply, the difficulties of choosing among the different solutions proposed by the experts underscored both the fiscal and scientific foundations of the controversy. Conflicting technical opinions and short-sighted ward politics continued to paralyze city government. Throughout this time the distinguished civil engineer John C. Trautwine advised controlling water use through meters, an unpopular proposal that might have made a difference, although Trautwine linked controlling waste to his support of purification through sedimentation rather than through more reliable, and more expensive, filtration. Eventually new bureaucratic and political perspectives on the authority of the mayor and responsibilities of the city produced the resources for an adequate supply of safe water for the entire population. Inevitable delays in establishing the complex system were further protracted by familiar partisan conflicts, but in 1906 a typhoid epidemic that killed 1,063 spurred action rather than debate.

McCarthy's differences with Warner over the origins and consequences of impediments to better public health in Philadelphia form a subtext to this fine study. To a large extent their differences grow from McCarthy's rich detail focused on the city's sanitary engineers and his talent in telling the story. McCarthy's emphasis on an earlier breakthrough for sanitary science rests partly on a questionable reading of the record for other localities. For instance, his example of high typhoid rates in Lawrence, Massachusetts, in 1892-93, despite the establishment of a famous experimental water filtration laboratory on the Merrimack River, fails to point out the ironic anomaly: sanitary legislation requiring filtration of public water supplies had virtually eliminated typhoid else-

where in the Commonwealth. Throughout the book there are serious typographical errors involving proper names and dates and troubling inaccuracies particularly with reference to contemporary scientific understanding and capabilities. A better index and a short description of Philadelphia's city government and population would make this absorbing monograph more accessible to readers.

The book is in most important respects a study to emulate. Its value is enhanced by attractive illustrations and publication in paperback.

BARBARA GUTMANN ROSENKRANTZ
Harvard University

BURTON W. FOLSOM, JR. *Entrepreneurs vs. The State: A New Look at the Rise of Big Business in America, 1840-1920*. Foreword by FORREST MCDONALD. Reston, Va.: Young America's Foundation. 1987. Pp. xii, 144. \$16.95.

The conservative resurgence of the 1980s has had minor effect on historical scholarship. An exception is a renewed emphasis on entrepreneurship. This book seeks to deepen that appreciation and to redirect it. Its thesis is that there are really two different types of entrepreneurs, one that is admirable and one that is not. The desirable kind, termed the "market entrepreneur," seeks to obtain wealth by competing in a free market with a superior product. The undesirable kind, termed the "political entrepreneur," seeks wealth by gaining access either to government subsidies or to a legally protected monopoly. If we read the historical record correctly, we will draw the lesson that the entrepreneur who seeks government subsidies seldom if ever provides a social benefit, while the entrepreneur who shuns government aid always does so.

The argument is elaborated in five case studies, each of which praises the contributions of a "market entrepreneur." In the purest statement of the theme, Cornelius Vanderbilt's reputation is resurrected. Rather than a river pirate, as in the Progressive version, Vanderbilt becomes a kind of white knight dedicated to the defense of economic freedom. Vanderbilt's superior efficiency slays the dragon of monopoly privilege as it appears in the form of Robert Fulton, Daniel Drew, and the Pacific mail lines. Although he was "personally repugnant," Vanderbilt's business activities were allegedly of great benefit to the nation (p. 15).

The subtlest essay makes James J. Hill an American hero as the only man who built a transcontinental railroad without government subsidy. The Great Northern was the only transcontinental that never went bankrupt, and Burton W. Folsom, Jr.,

believes that this proves the folly of using federal aid to encourage the construction of railroads ahead of market demand. Hill's line was planned thoughtfully, constructed carefully, and operated efficiently. The other three transcontinentals were sloppy jobs because the main goal was to feed on governmental largesse rather than to earn a legitimate profit.

The most daring attempt at revision is the author's paean to John D. Rockefeller. According to Folsom, after a brief flirtation with political entrepreneurship (the South Improvement Company), Rockefeller recognized the error of his ways. He became a sturdy market entrepreneur with a missionary commitment to bring cheap oil to the world. His wealth derived solely from superior efficiency with its concomitant social benefits. Rockefeller was a man of honor, serenity, generosity, and, most important, deep and genuine Christian commitment. Indeed, it is this great religiosity, the author hints, that is the real key to Rockefeller's success.

The book concludes with a sweeping indictment of mainstream historians. Textbook writers are denounced for routinely overlooking the contributions of entrepreneurs, presenting instead an unfair and inaccurate portrayal termed the "modified robber-baron" version (p. 105). Organizational history is reproved for its amorality; allegedly it deemphasizes the importance of individual action. Finally, contemporary social historians are scored for improperly minimizing the amount of social mobility in American society. (Horatio Alger is alive and well.)

In its slender archival research, simplistic analysis, and polemical tone, this book resembles some of the Progressive history it seeks to refute. If its level of analysis were to become the mode, the discipline would be set back decades. Few scholars will be persuaded by these arguments, but the book may be useful to those wishing to examine a revision of American history by a contemporary conservative.

STUART D. BRANDES
University of Wisconsin Centers

ANITA RAPONE. *The Guardian Life Insurance Company, 1860-1920*. New York: New York University Press. 1987. Pp. xiv, 209. \$35.00.

Those scholars who have become accustomed in recent years to the immense business history tapestries woven by Alfred D. Chandler, Jr., will be disappointed with this book. This study is a cameo—a short, well-written description of the development of a particular German-American enterprise. Anita Rapone carefully places her subject in

the context of late nineteenth- and early twentieth-century America, but she largely avoids generalizations and comparisons with other businesses (except those selling insurance).

The Guardian Life Insurance Company entered business in 1860 as the Germania Life Insurance Company. It began with a well-defined strategy of focusing primarily on the large and growing German-American population, a market niche that the company's directors (all of German background) and president, Hugo Wesendonck, understood very well. Using agents who worked for commissions, Germania soon developed a thriving business. Despite the problems created by the Civil War, Germania was by 1865 the twelfth largest insurance company in the United States, with \$15 million in policies in effect. A conservative, not an innovative, enterprise, Germania nevertheless continued to expand in the decades that followed, extending its business to Europe and Latin America. As it did so, the company gradually consolidated its operations (along lines similar to those followed by many other contemporary businesses) by improving internal communications and by transforming its agents into employees.

There is much in this saga that will interest historians of business, of ethnic groups, of women, and of social change. Germania was explicitly biased in favor of German-American employees, in favor of family members as managers, and against women and blacks as customers. Rapone does an excellent job of describing those and other company policies. She is obviously handicapped by the thinness of her sources, but she still gives her readers a good sense of what it was like to launch and steer an enterprise like Germania in the nineteenth-century United States. Her occasional naive statements about matters of business and economic history might have been eliminated if the project had used an advisory board (as many business histories do). But on balance this is a well-researched, well-organized, and carefully reasoned business history. The narrative ends in 1920 after a war-induced name change had taken place and the first signs of organizational ossification had begun to appear. Fortunately, we are promised a sequel that will tell how the Guardian revamped its operations and became by 1985 the twentieth largest firm in the business. I will await the next volume with great interest.

LOUIS GALAMBOS
Johns Hopkins University

ALLEN GUTTMANN. *A Whole New Ball Game: An Interpretation of American Sports*. Chapel Hill: University of North Carolina Press. 1988. Pp. x, 233. Cloth \$24.95, paper \$10.95.

In this, Allen Guttman's fourth book on sport, he states right away that he wishes to "bridge the gap . . . however slightly . . . between those who simply care about sports and those who care about them in a disciplined, reflective way" (p. ix). After some self-circumscribing "introductory definitions and paradigms," which are essentially a retelling and refining of the description of modern sport that he presented in his *From Ritual to Record: The Nature of Modern Sport* (1978), Guttman moves on to twelve brief chapters, each of which is a peculiar survey of an acknowledged problem in current academic research. The chapter titles are worth listing because they illustrate the breadth of current work on American sports and the breadth as well of Guttman's courage: "Pre-Columbian Sports," "Puritans at Play," "The Southerner as Sportsman," "The National Game" (baseball), "Muscular Christianity and the Point Spread" (basketball), "Children's Play from Jackson's Island to the Little Leagues," "The Anomaly of Intercollegiate Athletics," "Black Athletes," "Women's Sports," "The Destruction of the Body" (drugs), "The Rejection of Modern Sports" (sports criticism), and "The Future of American Sports." The bibliographical essay (pp. 209–19) is a generous survey of the large amount of work, much of it done in the last fifteen years, that has made possible essays as thought-provoking as those in the book at hand.

Unlike Guttman's biography of Avery Brundage, *The Games Must Go On* (1984), which is a cool narrative, this book is like Guttman's 1978 book and his *Sports Spectators* (1986) in that, though it is studded with illustrative anecdotes, it is essentially a series of critical surveys of the state of scholarship and intelligent opinion on all of the topics that the author has chosen.

It will appear to some readers that the ironic tone, the nice turns of phrase, and the steady flow of in-jokes are all attributable to the knowledge of the literature in several languages that is Guttman's special accomplishment. I rather believe that the assurance is more the result of the author's long-held theoretical position (still unusual in the United States) regarding the peculiar integration of sport in modern industrial society. An earlier, noble undertaking by Guttman was to translate the earliest (1969) and most vigorous statement of a German neo-Marxist critic regarding the co-evolution of modern sport and modern life. One can (perhaps should) examine Bero Rigauer's *Sport and Work* (1981). Often in the book under discussion here, the author praises sports-critical studies by a few German academic neo-Marxists of whom he says, "At the level of description (as opposed to judgment), the neo-Marxist

analysis of modern sport is fully congruent with mine" (p. 182).

But Guttman also congratulates many American scholars whose recent work is at a high level. Guttman's text is stimulating to read despite the fact that it is apart from the stale hagiography that characterizes both journalism and almost all the older academic writing about American sport. Guttman, like many others, is puzzled by the enormous amount of good literature on baseball and the lack of good secondary literature on other subjects such as basketball and intercollegiate sport. I believe that there is still not enough solid literature on the topics of the first three chapters and that the author would have been on more productive ground if he had ignored them and stayed with rather recent history where his theoretical position has already been so productive.

Guttman has "bridged the gap" only "slightly." It is difficult to envisage an audience other than a sophisticated academic one that will find this tour de force of much interest. The whole level of presentation is rich, allusive, ironic. I confess that it irritated me to know that some of Guttman's jokes simply passed me by. But any American sports academic (an increasingly numerous and productive breed) henceforth will have to be able at least to pretend to know the book thoroughly or merit shame or ridicule. The whole tone of work on American sport, at least at the higher levels of endeavor, has, with this series of essays by Guttman, been permanently elevated.

RICHARD D. MANDELL
University of South Carolina

ROBERT BOGDAN. *Freak Show: Presenting Human Oddities for Amusement and Profit*. Chicago: University of Chicago Press. 1988. Pp. xiii, 322. \$29.95.

Eli Bowen ("the Legless Wonder"), Clicko ("the Dancing Bushman"), and Ella Harper ("the Camel Girl") are not exactly names that routinely cross the pages of American history textbooks. By neglecting the story of "human oddities," Robert Bogdan suggests, historians have not only overlooked an important slice of Americana but also ignored an important institution, the freak show, that has left a lasting impress on American cultural values.

Bogdan is careful to define his terms at the start. "Freak," he asserts, "is a way of thinking, of presenting, a set of practices, an institution—not a characteristic of an individual." And the freak show is "the formally organized exhibition of people with alleged and real physical, mental, or behavioral anomalies for amusement and profit" (p. 10). Beginning in the 1840s and paralleling the

development of institutions like factories and hospitals, freak shows ceased being isolated attractions and became attached to dime museums, carnivals, and circuses. By the middle of the century, photographs of "freaks," taken and touched up by leading photographers such as Matthew Brady, had become collectibles and were featured on Victorian parlor tables. At the turn of the century, freak shows remained as popular as ever and were leading attractions along the midways at state and world fairs.

How and why did freak shows—the dust jacket calls them the "pornography of disability"—attain such legitimacy and respectability? Bogdan offers several explanations. Shrewd impresarios developed two modes of presentation that played on Victorians' concerns about well-being. An "exotic" mode, stressing alleged racial and cultural differences, pandered to the public's interest in hierarchies of human beings, while an "aggrandized" mode of presentation, affixing grandiose military and aristocratic titles (like General Tom Thumb) to persons on display, took advantage of widespread concerns among Victorian Americans about status. Strangely absent from these two prevailing strategies was pity, a sentiment that Bogdan traces to the decline of the freak show and to the growing control of human deviance by the medical profession in the twentieth century. Pity, he argues, is yet another form of presentation designed to enhance "the survival and expansion" of modern-day human service organizations. "In the end," Bogdan concludes, "the freak show has much in common with human service agencies" (p. 279).

Linking the modern-day human service agency to the freak show is a bold stroke and may well be correct, but Bogdan's assertion deserves the same kind of attention to archaeological detail that marks his analysis of the freak show itself. His pathbreaking archival research into the history and everyday operation of the freak show is extensive and has unearthed a treasure trove of information about the lives of exhibition managers, the lives of "freaks," and the show environment they jointly created in their efforts to dupe the rubes and exert a measure of control over their own lives. Although his analysis might have been strengthened by greater attention to Michel Foucault's theories of power relations and to the responses of American audiences to these presentations, Bogdan has written a book that will have lasting importance to historians of popular culture, science, and medicine. It also deserves an audience of persons concerned about the historical guises of disability. By reminding us of the proximity of the midway to main street, Bogdan

has made a notable contribution to American cultural studies.

ROBERT W. RYDELL

Montana State University

BETH L. BAILEY. *From Front Porch to Back Seat: Courtship in Twentieth-Century America*. Baltimore: Johns Hopkins University Press. 1988. Pp. x, 181. \$18.95.

This short, sometimes breezy, and generally engaging book draws on prescriptive literature to document two major changes in the conventions governing courtship in America between 1920 and 1965. First, the author discerns the movement of courtship from a female-dominated private sphere to a male-dominated public sphere. As dating replaced calling in the twentieth century, courtship was no longer a step on the road to matrimony but rather a means to achieve social standing in a new youth culture. Men's superior purchasing power allowed them to control dating, while ideals of femininity urged women to remain passive and concentrate on protecting their sexual virtue. Beth L. Bailey argues convincingly that dating became a "public commodity." She illustrates how the market became not only the locus of courtship but also a powerful metaphor for it; advice books spoke of "price tags," "supply," and "scarcity" to describe virtue as a "commodity," "merchandise," or "line" of goods (p. 95).

Second, dating itself evolved from one competitive system to another. Before World War II the "dating and rating" system prevailed in which stag lines of eligible young men competed for the dances and dates of young women, and the sheer number of dates determined status. After the war, "going steady" replaced the earlier "promiscuity." In this era of early marriage, students from junior high through college sought commitment and security in their "play marriages" with steadies. Bailey attributes this behavior to generalized fears of war and the bomb and argues further (though less convincingly) that a shortage of men during and after the war made women desperate about finding dates and husbands. Despite parental opposition, steady dating became the ideal until the sexual revolution of the 1960s.

Although she roots these new conventions "in all the -isms that fall under the mantle of modernization" (p. 119), Bailey points to more specific agents of change in the form of the new marriage education movement, an applied social science that, after 1920, flourished within colleges and filtered into public consciousness through popular magazines. Marriage educators, she argues, responded to modernization by attempting to cen-

tralize authority over personal life. Yet premodern societies also had central authorities, usually the clergy, who attempted to control personal life, and courtship often took place within a public sphere. More troublesome than this reliance on modernization theory is the assumption that dating was a national system and that the conventions of white, middle-class, largely college educated youth, the focus of most of her sources, were "dominant, even hegemonic" (p. 12). The book in fact suggests important changes in class relations within courtship, while it ignores distinctive regional and racial subcultures. Finally, although the author attempts to justify limiting the scope of this study to the "conventions" outlined in prescriptive literature, it would have been a much stronger book had she explored as well the rich behavioral evidence in social science literature and personal papers that might reveal how young men and women actually courted.

ESTELLE B. FREEDMAN
Stanford University

MARK FRIEDBERGER. *Farm Families and Change in Twentieth-Century America*. Lexington: University Press of Kentucky. 1988. Pp. x. 282. \$28.00.

Since the 1970s the economic crisis in American agriculture has steadily worsened. A precipitous decline in land values, low commodity prices, foreign competition, high operating costs, and a strong dollar have combined to force many farmers from the land. To make the situation more difficult, federal agricultural policy has been unable to meet the diverse needs of family farmers, while it has encouraged surplus production and allowed corporate farmers to take advantage of expensive price support and diversion programs. Major changes in science, technology, land tenure, inheritance, credit, and family and rural community relations also have altered traditional farm life during the late twentieth century. Even so, the current crisis in American agriculture stems, in part, from the tenacious desire of small-scale, family farmers to remain on the land, while many of them are no longer needed to maintain adequate agricultural production. Given the severe economic problems of American agriculture and the rapidly decreasing political power of farmers, one is tempted to contend that no solution to the farm problem is possible. Certainly, no single or easy solution exists.

Before there can be solutions, there must be understanding, that is, historical perspective. Mark Friedberger helps provide this perspective in a tightly focused study of farming in Iowa and California during the twentieth century. Although

he surveys twentieth-century developments, he emphasizes family farming during the 1970s and 1980s. Friedberger recognizes the geographic, economic, and cultural differences between these two states, such as land ownership and land-use patterns, labor requirements, and marketing and pricing structures. Yet he finds enough common ground to provide a broad survey of late twentieth-century farm problems. Friedberger correctly argues that bad judgment and miscalculation by farmers, creditors, and policy makers alike caused the current agricultural crisis. In addition, he specifically evaluates the manner in which technological change affected farming in Iowa and California's Central Valley, particularly in relation to increased specialization. Friedberger also analyzes land tenure, farm inheritance, and credit patterns. These chapters are the most important for providing new knowledge about little-understood aspects of American agriculture. Moreover, Friedberger employs the case-study method to evaluate the relationship of the family farm and farm community in rural society.

For the most part, Friedberger is optimistic about the future of the family farm, primarily because most people do not want to work as hard as farmers do for so little remuneration, while most farmers do not want other employment. Furthermore, farmers are a resilient people. No matter what the hardships, most of them prefer to endure because they like agriculture as a way of life. Unstated but implied in this work, however, is the reality that too many farmers still remain on the land. More farmers, most assuredly, will leave agriculture only to be replaced by neighbors who have a better capital base, a better credit rating, or better managerial ability.

Although Friedberger has a good understanding of twentieth-century American agriculture, his grasp of nineteenth-century developments is less sure. Consequently, his interpretation is arguable in places, such as his contention that the first agricultural revolution occurred in the New Deal years instead of during the Civil War. Moreover, farmers were at the mercy of market forces beyond their control long before the 1980s, as any study of the late nineteenth-century agrarian revolt will attest. These criticisms, however, are mere quibbles. Friedberger has provided a clear analysis of the agricultural problems in Iowa and California's Central Valley during the late twentieth century. No one has explained the inextricable link between farm and rural community better than he. This book is essential reading for anyone interested in the history of late twentieth-century American agriculture.

R. DOUGLAS HURT
Iowa State University

NORTH CALLAHAN. *Carl Sandburg: His Life and Works*. University Park: Pennsylvania State University Press. 1987. Pp. xiv, 258. \$29.75.

This gracefully written little book will not be of great interest to professional historians. Less a biography than a personal appreciation, it starts from the assumption that Carl Sandburg's six-volume work on Abraham Lincoln constituted "a monumental masterpiece" (p. 111).

North Callahan provides a pleasing, anecdotal account of the poet's early life, but the later parts of the book unravel into disconnected quotations about Sandburg's writings from friends and critics. Chronology suffers and so does believability once Callahan must deal with the period of Sandburg's fame, when he became an almost professionally eccentric and lovable rascal. Though aware of Robert Frost's warning that Sandburg was an "artificial and studied ruffian" (p. 85), Callahan rarely feels impelled to peek behind the mask.

Historians may find useful some of the quotations about Sandburg's work, many of them derived from the Carl Sandburg Collection at the University of Illinois. As Callahan never sets analysis as his main task, readers must assess for themselves the significance of these quotations.

From the materials found in this book, one could begin to understand Sandburg's shortcomings as a writer of history. He aimed "to break down all this sentimentalizing" about Lincoln (p. 105) but was himself a sentimentalist, albeit an anti-Republican and left-leaning one. Sandburg never leaned far enough leftward to question his own simple patriotism, first evidenced when he eagerly volunteered for the Spanish-American War. He left the Socialist party because he could not oppose World War I. He was never influenced by Mark Twain, probably too much a capitalist but surely also too much a cynic for this yea-saying midwestern bard. Sandburg's experience as a writer for various Chicago newspapers gave him only a journalistic appreciation for the day-to-day development of events, not an investigative curiosity or a critical approach to sources.

Callahan's book provides a rather sentimental look at a sentimentalist. Sandburg as a historian, at least, must be viewed more critically.

MARK E. NEELY, JR.

Louis A. Warren Lincoln Library and Museum

FRED HOWARD. *Wilbur and Orville: A Biography of the Wright Brothers*. New York: Alfred A. Knopf. 1987. Pp. xi, 530. \$24.95.

Fred Howard, who served as a member of the Library of Congress team that prepared the mon-

umental *Papers of Wilbur and Orville Wright* for publication in 1953, has provided us with a most satisfactory biography of the Wright brothers. Although readers will not find many new interpretations here, this book presents the story of the Wrights in rich detail not usually found in previous accounts. Moreover, the author succeeds in setting his tale within the context of life, technology, and politics in America and Europe at the turn of the century.

Howard skims lightly over those aspects of the story with which he assumes the reader will be most familiar; he covers the brothers' early years and the period of the invention of the airplane in the first third of the book. He offers a useful review of the technical issues, but those interested in a more detailed account should turn to Harry Combs's *Kill Devil Hill* (1979), Howard Wolko's *The Wright Flyer: An Engineering Perspective* (1987), or, best of all, Marvin McFarland's *The Papers of Wilbur and Orville Wright* (1953).

This biography comes to life as the author turns his attention to the period from 1905 to 1912, during which the Wrights struggled to market their invention and reap a just reward. Howard gives us a marvelous sense of the time and place, Europe and America at the peak of Edwardian splendor. He offers up rich characterizations of both the stars and the supporting players, from rival aeronautical figures, such as Augustus M. Herring, Glenn Hammond Curtiss, and Gabriel Voisin, to kings, presidents, and captains of industry.

The author walks us step by step through the maze of intrigue that preceded the first public flights of the Wright airplane in 1908. His interpretation of the Wrights' negotiations with the governments of the United States, England, and France is traditional. He offers the familiar portrait of two staunch and honest Americans beset by bureaucratic intransigence, double-dealing, and shortsightedness at the highest levels of government.

Howard provides a complete treatment of the Wrights' bitter legal struggle with Curtiss and others for patent protection and full recognition as the inventors of the airplane. The brothers won their battles in court but at a terrible price. Wilbur, his health undermined by stress, succumbed to typhoid in 1912. Orville fought on alone, feuding for twenty years with officials of the Smithsonian Institution who claimed that the 1903 Wright airplane had not been the first machine capable of flight.

Strangely, in view of the author's success in portraying the world in which the Wrights lived, his book is not completely satisfying as a biography. Readers are left with many of the most basic

questions unanswered. Why did Wilbur and Orville succeed where so many others had failed? What was the root of their genius? What forces shaped and drove them? Why does controversy continue to surround them? More than eight decades after the birth of powered flight, the two bicycle makers from Dayton, Ohio, continue to fascinate and intrigue us, while confounding and eluding those who seek to understand them.

TOM D. CROUCH
Smithsonian Institution

DAVID H. BURTON. *The Learned Presidency: Theodore Roosevelt, William Howard Taft, Woodrow Wilson*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1988. Pp. 225. \$29.50.

In a history of what he calls "the learned presidency," David H. Burton attempts to correct the "popular misconception" that men of learning do not belong in the White House. The stereotype is incorrect, Burton claims, because "at least a half-dozen of the forty or so presidents have substantial claim to being men of intellect, without forfeiting the right of being deemed men of action" (preface). The first three—John Adams, Thomas Jefferson, James Madison—constitute Burton's standard of reference; the other three—Theodore Roosevelt, William Howard Taft, Woodrow Wilson—his main subject. The book's leading purpose is "to analyze what they thought and what they wrote and how ideas shaped them as individuals prior to high-office" (preface); its secondary object is to consider the influence of their ideas on their presidencies.

Bringing together in one place closely related information from many different sources, Burton fulfills his principal intention in an interesting fashion. His accounts of the readings and writings of his subjects, as well as of the ideas that informed their intellectual development, make this book a useful reference. No reader familiar with the leading presidential biographies will, however, be surprised by its contents. Nor do Burton's analyses go very far in altering the popular conception of the presidency he wishes to dispel.

When one probes Burton's secondary subject, where alone an argument would seem to reside, that is, in the question of these three presidents' "transition of thought into action," the results are, once he gets past Theodore Roosevelt, mostly negative. Burton's own evidence often runs counter to his thesis that, "as the passive conception of the presidency common to the last century gave way to the dynamic executive leadership [*sic*], the transition was shaped and directed by men of the mind" (preface). Taft, for example, was a

notably passive president, obliging Burton to devote most of his discussion (eight pages to one) to Taft's "active" career as a jurist. While president, "Taft was," he admits, "politically incapable of following through on actions he was intellectually convinced were necessary" (p. 114). Similarly, Wilson's learning did not overcome other defects: "His refusal to take any senators with him to Paris, much less any Republican senators, flew directly in the face of both common sense and constitutional reality. His learning in the field of politics served him poorly in all these decisions" (p. 188).

Although the writing is, in general, adequate, too many pages are marred by the kinds of problems exhibited in one of Burton's concluding passages: "What a close consideration of Roosevelt, Taft, and Wilson yields is not only learned presidents but 'the learned presidency.' Because it is viewed in a new light the post takes on a new dimension. A quasi-biographical examination of the writings of these presidents imparts a learned proportion to the office" (p. 191). A close consideration of these men cannot "yield" either learned presidents or a learned presidency; the next sentence is tautological; an examination of presidents' writings cannot "impart" learnedness to the presidency; and imparting "a learned proportion" to anything does not make sense.

JUDITH ICKE ANDERSON
California State Polytechnic University,
Pomona

H. M. GITELMAN. *Legacy of the Ludlow Massacre: A Chapter in American Industrial Relations*. Philadelphia: University of Pennsylvania Press. 1988. Pp. xv, 355. \$29.95.

This book is an account of the aftermath of the infamous Ludlow massacre of April 20, 1914—not what transpired in the coal fields of Colorado but what took place in the board rooms of corporate America. It is a history of the origins and early development of employee representation, an important strain of corporate reform that emerged from the bloodstained fields of Colorado and achieved its heyday in the 1920s.

Because the Rockefeller family had a controlling interest in and was so closely identified with the largest and most visible of the coal firms publicly implicated in the massacre, the Colorado Fuel and Iron Company, and because the events of April 1914 were so politically and emotionally charged, it is not surprising that the Ludlow massacre became a catalyst for a reevaluation of industrial relations policy by John D. Rockefeller, Jr. Rockefeller's recognition that he needed to assuage public condemnation (as well as his con-

science) and to ensure future labor peace ultimately led to his adoption and promotion of an employee representation plan, the Colorado Industrial Plan.

Guided throughout by the Canadian labor relations reformer Mackenzie King (former minister of labor and future Canadian prime minister), Rockefeller came to develop a special and quite intimate relationship with the Canadian labor expert. It is this story—of the emotional and intellectual bonds that linked King and Rockefeller and led them, albeit temporarily, to a common vision of industrial relations reform—that H. M. Gitelman tells so well. King, the peripatetic Christian socialist statesman, possessed somewhat naive views of the roots of social conflict and tended to idealize the working class. Both intellectual bents permitted him to compromise and rationalize his work in the service of capital. He believed he could influence Rockefeller and thus possibly shape the course of industrial relations for years to come. But King's hopes for employee representation came to naught; Rockefeller and other capitalists had no intention of permitting it to be the bridge to union-management cooperation that King had hoped it would be. Yet whatever disappointments King faced in his work on corporate reform were tempered by the anticipation that his destiny lay elsewhere—as it did—in Canadian politics.

Rockefeller, however, achieved his personal salvation through reform. Aply stripping away the veneer of Rockefeller's public persona, Gitelman discloses the vulnerable son struggling to create an identity for himself and succeeding in the role of a corporate liberal. But, having achieved his freedom from his domineering, conservative father by establishing his credentials as a labor and social reformer, Rockefeller would go no further. He never transcended the halfway reforms of the Colorado Industrial Plan, the vehicle of his personal psychological liberation.

Gitelman's heavy reliance on King's diaries and Rockefeller's papers suggests both the strengths and limitations of this book. The level of intimacy achieved by the author is accomplished at the expense of context and horizon. Although the subtle twists and turns of daily encounters, personalities, and psychological motives of the principal actors are well examined, significant aspects of the story are neglected. As the author himself observes, on a public level employee representation developed a "life of its own" (p. 303), apart from the machinations of Rockefeller and King. But, unfortunately, this broader and more public "legacy" of Ludlow figures as only a small part of Gitelman's story. It should have been more prominent.

Although there are additional imperfections in

this book—most easily corrected by the pruning hand of an aggressive editor (too much repetition, too many textual explications and summaries, and far too many conversations and meetings recounted in painful detail)—it does make a solid contribution to our understanding of King, John D. Rockefeller, Jr., and the origins and early years of company unionism in the United States.

GERALD ZAHAVI
State University of New York,
Albany

JO ANN E. ARGERSINGER. *Toward a New Deal in Baltimore: People and Government in the Great Depression*. Chapel Hill: University of North Carolina Press. 1988. Pp. xix, 284. \$29.95.

This study contains the kind of sure research and skillful analysis that the story of the Great Depression and the New Deal experiment deserves but that is achieved so rarely. Jo Ann E. Argersinger's immediate subject is the fate of the unemployed in the city of Baltimore, Maryland, during the 1930s. She examines the dynamic of national and local relief programs and the politics of the bureaucracies that supported or resisted them. Her book sets new standards for measuring the social impact of public policy. Argersinger's careful scholarship and constant comparison of local to national, of policy expectation to program result, make this study of Baltimore a hallmark of state and local history and an important addition to the historiography of the New Deal.

Concurring generally with the arguments of James T. Patterson, Charles H. Trout, and James Wickens that the New Deal's relief and recovery programs were thwarted in the states and cities of the nation by combinations of local tradition, fiscal conservatism, political rigidity, ethnic rivalry, and assorted private and public bureaucracies, Argersinger examines the impact of federal relief in Baltimore by specific attention to citizen participation and community organization.

Argersinger's typology suggests that we would do well to examine the dynamic between those citizen groups encouraged and those restricted by the federal government's policies in our explanations of why some New Deal programs achieved more acceptance than others. Her conclusions about the role of citizen activism also illuminate the quixotic fortunes of municipal candidates, particularly Baltimore's fractious Democrats. That city's experience during the New Deal indicates that historians have been too quick to judge the outcome of relief programs. Argersinger shows convincingly that citizen participation, organiza-

tion, and pressure as well as municipal sabotage and resistance are critical ingredients.

Drawing on a variety of oral and written evidence and subjecting her findings to regression analysis, Argersinger argues that federal intervention elicited a complex and unpredictable response from the organized (the political parties and labor unions) as well as from the unorganized (the poor, females, blacks, and the homeless). Her evidence is particularly important for understanding the reaction of women, blacks, and seamen to local relief programs. She exposes the influence of the new social-work professionals who administered the programs and determined the qualifications of relief recipients.

Argersinger's chapters on labor organizations and Baltimore's meager public housing programs are especially good. In these areas of relief policy, her assertion of the centrality of the unorganized to resist or modify bureaucratic mandates and personnel is most persuasive. If her analysis of public housing and gender-described relief programs is accurate, it ought to revise substantially the notion that New Deal policies ignored minorities and force reconsideration of what specific groups did in other cities to intrude themselves into the political process.

Argersinger's research in primary records, personal papers, and government archives gives her study of Baltimore special richness. She notes that Baltimore was the only city in the nation to secure an arrangement whereby funds from the Works Progress Administration (WPA) were combined with those from the Public Works Administration (PWA) to finance large construction projects rather than local relief. This unusual capitulation of federal authority gave Baltimore Democrats financial flexibility in construction projects, fewer WPA projects, and more angry voters. Argersinger's analysis of PWA projects is too brief, but it suggests that the failure of the program may have resulted as much from local subversion as from the penurious directives of Harold Ickes.

Except for her obvious concern for the plight of the unemployed, Argersinger writes with cool detachment. Her story is peopled with characters whose motives she describes in staccato. One longs for fuller portraits of Governor Albert Ritchie, of party chief William Curran, and especially of Mayor Howard Jackson, whose political ideology she describes as "utterly bankrupt" (p. 207).

Although the New Deal made only modest changes in the structure of politics in Baltimore and in the lives of most of its citizens, Argersinger shows us that this conclusion is ambiguous. She reveals new dimensions of subtle grass-roots change. The critical difference that citizen participation made in Baltimore should cause us to

reassess some of our most cherished notions about the impact of the New Deal.

LINDA J. LEAR
American University

CHRISTOPHER H. JOHNSON. *Maurice Sugar: Law, Labor, and the Left in Detroit, 1912-1950*. Detroit: Wayne State University Press. 1988. Pp. 334. Cloth \$39.95, paper \$15.95.

This biography of Maurice Sugar, a leading labor lawyer and political activist who was general counsel of the United Auto Workers (UAW) from 1939 to 1947, seeks to rescue from obscurity an important figure in American labor history and to illuminate the history of the Communist-led Left in Detroit in the 1930s and 1940s. The study is based on research in papers housed at the Walter Reuther Library and at the Goodman, Eden, Milender, and Bedrosian law firm, on an incomplete autobiography, and on oral interviews. Christopher H. Johnson portrays Sugar as an "independent Marxist of generally pro-Soviet leanings" (p. 12) who, as the highest placed pro-Communist in the UAW, presided fairly and effectively over union interests and democracy until he was ousted in 1947. Johnson also depicts a vital Communist-led labor movement and community in Detroit that, during its heyday, helped build the foundations of auto unionism and stirred the beginnings of an interracial civil rights struggle in the city.

Sugar, the son of Jewish immigrant parents, was reared in Michigan's Upper Peninsula timber country and came of age in early twentieth-century, booming Detroit. A graduate of the University of Michigan Law School, he became first a Debsian socialist, labor lawyer, and antiwar activist and then, in the 1920s, a Marxist-Leninist. In the 1930s, he emerged as a key figure in the Communist-led segment of Detroit's labor movement—a labor defense lawyer and activist in party fronts who worked in 1933-34 with the Mechanics Education Society, one of several UAW forerunners, helping it confront the early New Deal auto code. He then ran repeatedly for local office in 1935-36, energizing a broad Left community comprised of working-class, ethnic, and racial organizations, and he subsequently served as a key adviser in the historic auto union recognition struggles in the late 1930s and early 1940s. A fascinating portion of the book reviews Sugar's role as political strategist and legal tactician during the famous sitdown strikes at General Motors in early 1937 and as UAW legal counsel prior to and during World War II.

Johnson succeeds most in bringing to life the young Sugar and in tracing the key questions that

a Marxist labor lawyer confronted whose career spanned several decades of Left activity, the rise of the New Deal, the growth of auto unionism, and the eruption of factional conflict in the UAW. Johnson credits the role of pro-Communists and also socialists who helped build industrial unionism. He also emphasizes the crucial role the New Deal state and law played in shaping labor relations over two decades. His study, however, is not without some flaws. The older Sugar disappears as a personality after 1938, owing to a paucity of documentation. Moreover, Johnson never pins down adequately Sugar's precise relation to the Communist party. I believe the evidence suggests that Sugar was more than a "Debsian Socialist who appreciated the U.S. Communist movement" (p. 299)—no doubt more, too, than "a sympathetic nonmember" or fellow traveler (p. 12). Johnson also draws on few voices among the Reuther forces who battled against Sugar and his allies over the years, and he mostly asserts that Sugar was victimized by the factional warfare, playing down his and the party's roles as active participants. Labor historians will benefit from knowing the Sugar story but will discover little that is novel about the UAW's evolution or about law and labor relations during or after World War II. The riddle of Sugar's withdrawal from the Left after 1947—he sat out Henry Wallace's campaign, abandoned Detroit and the law firm he had founded, and retired to rural Michigan—also remains a mystery.

KENNETH WALTZER
James Madison College
Michigan State University

GILBERT J. GALL. *The Politics of Right to Work: The Labor Federations as Special Interests, 1943–1979*. (Contributions in Labor Studies, number 24.) New York: Greenwood. 1988. Pp. xiii, 244. \$39.95.

This book is an interesting, thoroughly documented, and provocative study of the politics of the "right to work" issue at the state and national level from the early 1940s to the late 1970s written from the perspective of organized labor. Beyond providing a useful chronological account of what occurred when the right-to-work issue became immersed in the political process, Gilbert J. Gall analyzes and assesses organized labor's ability to impose a special-interest influence on American party politics, particularly on the Democratic party.

Documentation comes from traditional sources, such as the archives of labor organizations, papers of politicians, and, to a lesser extent, the records of supporters of right to work, both individuals and organizations. Gall's interpretation is also based on

an innovative use of the Rice Index of Cohesion, a statistical technique of legislative role-call analysis.

Gall begins with the origins of the right-to-work movement in the 1940s as organized labor lobbied successfully for union security by government decree (War Labor Board) in exchange for wage restraint during World War II. Highlighted is the development by labor's opponents of the brilliant strategy of ceding jurisdiction over the question of union security to the states through the mechanism of Section 14(b) of the Taft-Hartley Act and then submitting the issue to popular referendum in selected states where the inspired misnomer "right to work" was as great a deceiver as the phrase "employer 'free speech.'"

The 1950s were loaded with revealing and important events including unsuccessful efforts by George Meany, president of the American Federation of Labor (AFL), to negotiate a deal with the National Association of Manufacturers despite professing his commitment to capitalism and belief in "management's right to manage." The conservative AFL building-trades unions also attempted to make their own deals (the elimination of mandatory injunctions in secondary boycott cases, for example) with an Eisenhower administration seeking to avoid permanent minority party status by demonstrating that it was not antilabor.

One major theme of lasting historical and political importance emerges: organized labor's movement away from the AFL's policy of nonpartisan rewarding of friends and punishing enemies and of political involvement only to protect the freedom to engage in self-help measures to pursuit by the merged American Federation of Labor–Congress of Industrial Organizations (AFL-CIO) of an alliance with the Democratic party. Despite organized labor's contributions to the victories of John Kennedy, Lyndon Johnson, and Jimmy Carter, however, the AFL-CIO was unable to get Section 14(b) repealed or any other significant reform law passed. These goals were thwarted not only by labor's traditional Republican and southern Democratic opponents but also by Democratic administrations that would not risk jeopardizing their domestic programs and foreign policies by crusading for divisive "special-interest" issues such as repeal of Section 14(b). Ironically, it was the strength of the AFL-CIO's ties to the Democratic party that left the federation with no realistic alternative to the party and made it obvious to Democratic strategists that any abandonment of the party by the AFL-CIO would be short-lived. As the author puts it, Democrats "doubted the federation's ability to inflict direct retribution" (p. 156). Was the abandonment of voluntarism a mistake?

Gall's conclusions concerning organized labor's political influence in these matters are, however,

more sanguine than the evidence justifies. He claims that the AFL-CIO did benefit from a "fruitful partnership" with the Democratic party (p. 10), but he is not explicit about the nature of those benefits, except to say that organized labor's political activity prevented more severe constraints. If political influence is measured by the ability to pass legislation over opposition, however, the AFL-CIO's alliance with the Democratic party has been a failure on the right-to-work issue. Organized labor has not had a handle on the federal government since the early Wagner Act days.

As the author notes, David Feller, once a general counsel of the AFL-CIO, warned over twenty years ago that repeal of Section 14(b) was the wrong issue; it had some psychological but little practical value. Feller argued that delays in handling cases by the National Labor Relations Board, employer "free speech," and the rights of strike replacements were more important and practical issues. Given organized labor's poor public image, was it a mistake to concentrate on an issue that may have been unimportant and certainly raised suspicions (justified or not) of coercion of individuals and an organizational power grab by unions? It would have been interesting and useful to have Gall's evaluation of Feller's arguments.

But these are only minor reservations and reviewer's quibbles. This is an excellent book for scholars of political science, government, and history as well as industrial relations specialists.

JAMES A. GROSS
Cornell University

PHILLIP MCGUIRE. *He, Too, Spoke for Democracy: Judge Hastie, World War II, and the Black Soldier*. (Contributions in Afro-American and African Studies, number 110.) New York: Greenwood. 1988. Pp. xvii, 154. \$35.95.

A leading voice for the black soldier during World War II was Judge William H. Hastie, civilian aide to the secretary of war from November 1940 to January 1943. Hastie's role has been discussed in many works dealing with the period, but Phillip McGuire brings a well-researched, fresh perspective to the topic.

McGuire presents a solid discussion of Hastie's role during the critical period when the War Department was formulating policy on the use of black soldiers. This short, rather expensive work chronicles Hastie's battles with the War Department's bureaucracy while blacks were battling the entrenched segregation and racism of the military and American society. What McGuire brings to the forefront is the problem of a minority in a

hostile bureaucracy attempting to make changes. Hastie often acted alone within the bureaucracy. But, with strong support from blacks and whites outside it, he was able to exert pressure at critical moments and to bring about some changes. Examples include the commissioning of more black medical personnel, increased opportunities for blacks at Tuskegee Air Base, better protection for individual black soldiers, and more War Department news coverage in the black press.

The degree of Hastie's success is difficult to evaluate and subject to interpretation. McGuire calls him a "catalyst" for change (p. 110) and his role "in occasioning the beginning of this new socio-military attitude" indisputable (p. 97). The sad irony of Hastie's career is that he may have created more change by resigning than he was able to effect while serving. His successor, Truman K. Gibson, Jr., was able to accomplish more in the War Department because of the impact of Hastie's resignation and because Gibson worked more effectively within the military bureaucracy.

Although McGuire tends to place Hastie on a pedestal, the book provides good coverage of Hastie's career and work.

ALAN M. OSUR
National Systems and Research Company
Colorado Springs, Colorado

MARY MARTHA THOMAS. *Riveting and Rationing in Dixie: Alabama Women and the Second World War*. Tuscaloosa: University of Alabama Press. 1987. Pp. x, 145. \$16.95.

All wars cause social and economic upheavals, but no conflict was more disruptive than World War II, which changed forever America's demographic, social, and cultural patterns. One of the most significant changes occurred when thousands of women entered the nation's work force and took over previously male-only tasks, an event immortalized by the popular song "Rosie the Riveter." Women of every age, race, and economic status who had never worked outside the home before left kitchens, gardens, and families to become wage earners. By 1944 seven million females, 36 percent of the nation's workers, were wiring airplane cockpits, sewing parachutes, riveting fuselages, modifying tank turrets, welding bulkheads, and assembling bombs. They were also holding down positions as nurses aides, Red Cross volunteers, canteen operators, USO hostesses, government secretaries, and ration book administrators. This phenomenon, while national in scope, nevertheless produced different effects in different parts of the country. Mary Martha Thomas, in her delightfully titled book, explores how

women in the heart of the Deep South took up the challenge.

Alabama was one of the most industrialized states in the South, and, with steel mills in the Birmingham area, shipyards on the coast, arsenals near Huntsville, and aircraft and military facilities throughout, there were ample job opportunities for women. For white women, that is. Black women lived in a separate world. Thomas does a fine job describing the effects of that separation. Using contemporary government reports and statistics, the state's newspapers, and women's own voices, she describes what it was like for all women who joined the war effort in Alabama. From her research she draws several major conclusions. First, because of Alabama's conservative social values and its grassroots support of traditional family roles, middle- and upper-class women favored volunteer jobs rather than the heavy-duty industrial jobs urged on them by the government. This caused many of the state's defense industries to face critical labor shortages for the duration of the war despite the fact that thousands of Alabama women did respond to the government call. In addition, Thomas shows that, because of racism, black women did not reap the job or wage benefits that accrued to their white sisters. On more than one occasion the government's Fair Employment Practices Commission had to sue to get black women training programs, jobs, and prevailing wages, and even then the majority of black women remained outside the mainstream in low-paying service jobs. Finally, Thomas concludes that as soon as the war was over it was back to business as usual in Alabama. The war seems to have had no positive, long-lasting effects on the local public's perception of women's roles in society—an unfortunately meager legacy for such a watershed period.

The book, which is well researched and written, includes chapters that cover the mobilization's effects on life in Alabama, how women were recruited and trained, the kinds of defense jobs they acquired, how the companies, the public, and women themselves adjusted to wartime developments, and the kinds of volunteer activities the women did. Thomas includes a chapter on housewives, shortages, and rationing. The reader will especially appreciate two other chapters: a conclusion in which Thomas sums up her research and her findings, and, an essay on sources, which surveys past and present literature on the subject. The book also includes photographs, notes, and a useful index. I recommend this work to all scholars interested in women's studies and in the history of the South.

LINDA VANCE
Austin, Texas

LEWIS A. COSER. *Refugee Scholars in America: Their Impact and Their Experiences*. New Haven: Yale University Press. 1984. Pp. xviii, 351. \$25.00.

Lewis A. Coser's study offers a collective portrait of fifty European scholars who came to the United States following Adolf Hitler's seizure of power in 1933. Coser, a distinguished professor of sociology and a refugee himself, describes his book as a "history of ideas" that seeks to determine to what extent these intellectuals influenced American culture and why some exercised great influence while others suffered almost complete neglect. Indifferent to narrative, chronology, and political context, Coser divides his story into occupational categories—psychology and psychoanalysis, sociology and social thought, political science and political theory, humanities, and theology. For reasons of personal interest and practicality, he does not include discussions of scientists or artists, those scholars who failed to influence American thought, nor those who found professional niches in the South or Midwest.

With the exception of a survey of psychoanalysts, Coser undertook no statistical surveys or archival research. Only occasionally did he conduct oral interviews with surviving refugees. Virtually all of his research derived from secondary works. His book, however, has a more serious problem. Coser writes what he describes as a "history of ideas," which he curiously defines as "people and their relationships with their fellows" (p. xi). Coser's book has value but not as a history of ideas. Innocent of analysis, he offers only sketchy descriptions of individuals' theories, often no more than one or two sentences. His study is in truth a sociology of ideas.

Interested in the extent to which particular refugee scholars contributed to American scholarship and culture, Coser found that "success" more often accrued to those who acculturated themselves to American society or who attached themselves to existing professional networks. Those who cloistered themselves with other refugees or who failed to acquire positions at prestigious institutions had little opportunity to influence American thought and culture. Refugees who received appointments at prestigious institutions or in other ways joined the "old boy" networks of American scholarship virtually assured themselves of influence. So, too, refugees enhanced their likelihood of influencing American culture if they learned the language and studied the nation's history.

As a partial explanation of why some refugee scholars succeeded and others failed, Coser's "professional networking" argument is useful. For him, however, it is the whole story. As he presents

it, the history of ideas is little more than academic influence peddling. Private clubs, old school ties, and ethnicity become more effective predictors of intellectual influence than rigorous, provocative, or brilliant thought. He confuses professional prestige and awards with intellectual significance. No doubt, knowing the right people, attending the right schools, glad-handing at professional meetings, and other forms of networking yield enormous professional dividends. Networking secures tenure and promotion, publication by prestigious journals and presses, research grants and fellowships, and election to honorary scholarly offices. Professional success and prestige are quite different, however, from intellectual eminence.

Hitler cut short the careers and even the lives of many potentially powerful thinkers. Similarly, anti-Semitism and the clubbiness of American academic life demoralized a number of significant scholars. Even so, miraculously, the work of the best thinkers, such as Hannah Arendt, Leo Strauss, and Erik Erikson, was salvaged, not because of the refugees' "relationships with their fellows" (p. xi) but because their ideas altered the way we understand reality.

WILLIAM B. SCOTT
Kenyon College

NAOMI W. COHEN. *The Year after the Riots: American Responses to the Palestine Crisis of 1929–30*. Detroit: Wayne State University Press. 1988. Pp. 210. \$24.95.

An aloof president who believed he could coopt silence from a divided, dilatory minority, an American Jewish community that was "powerless" before an insensitive State Department, the blood of innocents shed for no other reason than that they were Jews—all of this should make for familiar reading. And, indeed, Naomi W. Cohen notes parallels between mistakes made by American Jews and their friends during the Holocaust and those perpetrated in the year following the Wailing Wall riots of 1929. In that earlier peacetime era, however, Jews dissipated what Cohen believes was an excellent opportunity to influence public opinion and shape Anglo-American policy in the Middle East. National organizations failed to coordinate spontaneous demonstrations at the local level. Zionist leaders—Louis Lipsky, Louis D. Brandeis, Chaim Weizmann—were so swept up in squabbles over position that they never mounted a concerted public information campaign. Establishment groups such as the American Joint Distribution Committee threatened to withhold funds unless the Jewish Agency endorsed its "peace now"

rhetoric. As in 1942, 1948, 1967, and 1973, Jews naively placed their faith in "purist-legal" documents and facts, hoping that reason would persuade British officials and Arabs to be fair.

One hundred thirty Jews died in riots orchestrated by Haj Āmin al-Ĥusaynī, the Grand Mufti of Jerusalem, a man who was once the most charismatic Arab nationalist, and all that the Zionists received in return was the Passfield White Paper that limited immigration and land acquisition. Drawing extensively from government cables and private reports, Cohen posits villainy and ineptitude among a number of prominent Americans. These include U.S. Consul General Paul Knabenshue and newspapermen Joseph Levy and Vincent Sheean, who deliberately slanted their assessments of Holy Land atrocities; State Department bureau chiefs G. Howland Shaw and Wallace Murray who echoed the coarse anti-Semitism of their British counterparts; Felix Warburg, a quixotic Jewish millionaire whose intentions were good, if flawed; and Judah Magnes, president of the Hebrew University of Jerusalem, who may have done more harm to Jewish aspirations with a single speech (November 19, 1929) and article ("Like All the Nations" [1930]) than any bigot.

In a manner reminiscent of earlier authors who have dealt with American reaction to the Mortara affair and Kishinev pogrom, Cohen provides in-depth analysis of the generally hostile (anti-Zionist) positions of the Protestant and Catholic press, liberal journals, Jewish Bundists, and Stalinists in America. Much like Malcolm Hay, Cohen recounts, and dismisses, facile arguments of church and government ministers who denounced Zionists as arrogant Bolsheviks with money. Quite correctly, she views the Passfield White Paper, like the Churchill White Paper of 1922 and the later White Paper of 1939, as a stepping stone in Britain's abdication of its responsibilities under the League of Nations Mandate. Regrettably Cohen devotes less than a page to the response of Jabotinsky's Revisionists, the very group so often blamed for "inciting" the Arabs. Most perturbing, after 174 pages telling how Jews repeatedly blundered, she leaves the reader wondering how and why the government of Ramsay MacDonald "backtracked" on the Passfield White Paper.

This important work, a primer in Jewish anomie, should go far in dispelling notions that British officials or any American secretary of state (Charles Evans Hughes, Frank B. Kellogg, and Henry L. Stimson included) favored the Zionists during the interwar period.

SAUL S. FRIEDMAN
Youngstown State University

MARC S. GALLICCHIO. *The Cold War Begins in Asia: American East Asian Policy and the Fall of the Japanese Empire*. (Contemporary American History Series.) New York: Columbia University Press. 1988. Pp. xvi, 188. \$25.00.

In this slender monograph Marc S. Gallicchio focuses on one year, 1945, which marked the end of war and the beginning of cold war in Asia. Studying one year, particularly one so epochal, has attractions for a scholar, but it does bring problems—a certain abruptness, a strained quality to the thesis, a lack of precursors and aftermath. Extensive documentation and familiarity with the relevant secondary literature strengthen the work, but Gallicchio adds no important reevaluations or reinterpretations. This is well-crafted but unsurprising work, contributing nuances, reexamining traditional questions, and supplying a wealth of minutiae from the occasional new sources here unveiled.

Yalta set the parameters for decisions regarding East Asia, and Gallicchio maintains that Harry Truman read the agreement as early as May. Intending to stay within the limits of Franklin Roosevelt's policies, Truman relied increasingly on the strongly anticommunist State Department and within a month began to deviate from Roosevelt's anticolonialism and cautious diplomacy. But Truman still wanted a free, friendly China and a nonrevolutionary Korea and Indochina.

China, a wartime imbroglio despite Roosevelt's "largely symbolic" policy, remained the major problem, and 1945 brought no resolution. Gallicchio doubts that Roosevelt perceived Joseph Stalin's agreement to support the Nationalists as an attempt to contain Chinese communism by denying it outside assistance. He maintains that Truman reluctantly backed Chiang Kai-shek but lacked good intelligence after Patrick Hurley purged the China hands from the U.S. mission. John Carter Vincent was more knowledgeable than Hurley, portrayed here as a competent diplomat, but Vincent's advocacy of American impartiality and Kuomintang reform would have disturbed the status quo.

The author stresses the disjunction between political and military policy. Truman did not challenge what is described as Soviet (not Chinese) predominance in northeast China, but success at Alamogordo led him to push for speedy Japanese surrender without the Soviets. George Marshall, oddly, did not try to convince Truman and Secretary of State James Byrnes to clarify the emperor's position in the Potsdam Declaration, which might have hastened surrender. Truman's apparent lapse is explained thus: he doubted the declaration alone would work, an imperial reference

would entail political risk, and he believed the bomb would end discussion anyway.

The United States was ill prepared for a sudden surrender in any case. China now posed greater problems than Japan—with incipient civil war, Japanese troops adrift, and Soviet forces nearly ready to enter Manchuria. Hastily drafted, the general order for Japan's surrender was really a continuation of existing military boundaries with strong political implications.

The author notes some successes: unilateral occupation of Japan by Americans and Soviet occupation not of all of Korea but just to the thirty-eighth parallel. But the United States failed to obtain Dairen and landing rights in the Kuriles. Friction with Britain over Korea, Hong Kong, and Indochina was relatively minimal. Marines in China aided in repatriating Japanese forces, supported Chiang, and showed the flag to the Soviets. But Gallicchio contends a mistake was made in assigning northern Indochina to the Chinese instead of pushing for a speedy French return, and the Chinese harmed both the French and the Vietnamese. Again the fault of haste.

By autumn Soviet intractability and the Chinese Communist party's increasing strength brought fears of the Marines' potential involvement in civil war. Byrnes attempted to redefine China policy as Vincent had recommended, fearing more aid to the Nationalists would lead to Soviet countermeasures. Hurley's resignation intensified the debate, and as the year ended Truman sent Marshall to China and prepared to replace Byrnes, whom he saw as too conciliatory toward the Russians.

Gallicchio concludes that Truman abandoned the Grand Alliance in order to obtain favorable outcomes in China, Korea, Japan, and Indochina. He was thwarted by his former allies and undone by rapidly changing and unforeseen events. China policy was in limbo; America pursued limited intervention, not disengagement and compromise, and only later adopted a defensive perimeter strategy. In Indochina, Truman, hampered by anticolonialism, unwittingly aided the survival of the Vietminh (a startling conclusion). U.S. policy toward Japan, little discussed, awaited Douglas MacArthur. Truman, anti-Russian and anticommunist but unsuccessful in Asia, presided over the beginnings of the militarization of American foreign policy.

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ARNOLD XIANGZE JIANG. *The United States and China*. (The United States in the World: Foreign Perspectives.) Chicago: University of Chicago Press. 1988. Pp. xiii, 200. \$22.00.

This book is part of a series edited by Akira Iriye, who believes that these volumes, written by distinguished foreign authors, will compensate for the "uninational outlook" of too many American scholars and teach us more about "how foreigners view and deal with the United States" (p. ix). The effort to move beyond the American perspective is laudable, but using foreign authors to do so is no guarantee that the books they write will be the equal of the many remarkably fine studies produced by American scholars specializing in American-East Asian relations.

Arnold Xiangze Jiang's book is a case in point. In 172 pages of text, he seeks to trace the long, complicated evolution of Sino-American relations. Jiang has read widely in American primary sources and secondary works and often provides a concise, intelligent analysis of policies and events as well as a useful elaboration of some general themes. He emphasizes America's pursuit of its self-interest in China in the nineteenth century, noting that the United States collaborated with the other Western powers in reducing China to semi-colonial status. After the turn of the century, the United States often took the lead in trying to impose stability on China and to ensure, through balance-of-power diplomacy, that no one power dominated that nation. The Open Door Notes, Theodore Roosevelt's diplomacy, and the Washington Conference sought to advance these goals. Increasingly, as Jiang points out, the United States became entangled with corrupt, repressive, and unpopular regimes, eventually tying itself to Chiang Kai-shek and his Nationalist government. American policy makers, in seeking to preserve order and treaty rights in China and later in seeking to use Chiang as a vehicle through which to expand American influence in East Asia, were caught in a series of contradictions and misperceptions, misreading popular currents in China and the nature of the revolution there.

Although these themes are accurate as far as they go, they add little to our knowledge of Sino-American relations, and the omissions and biases of the book are striking. Jiang does not mention the role of American missionaries and largely ignores Sino-Soviet relations and the internal history of the Chinese communist movement. His book is thinnest at exactly those points where one would expect him to know the most. Moreover, many of his judgments reflect a standard party line. After the founding of the Chinese Communist party in 1921, the reader learns that "the Chinese people enjoyed the leadership of a party that guided them in the correct direction and fought unflinchingly in the forefront of their struggle against the powerful imperialists and their lackeys in China" (p. 82). The Chinese Com-

munists are without a blemish, while Chiang Kai-shek lacks any virtues or achievements. Jiang dismisses him as a "mixture of warlord, stockbroker, and underworld boss" (p. 104), a harsh dictator without any popular support or an alternate vision of the Chinese revolution. His treatment of the Korean War is equally simplistic. After the initial invasion of South Korea (for which he gives no explanation), Jiang claims that the United States, through its ties with Taiwan and the invasion of North Korea, threatened China with war. Had Chinese volunteers not entered the fighting and inflicted a severe defeat on American forces, Jiang wonders if Harry S. Truman would have recalled Douglas MacArthur and fought a limited war.

Too often, then, Jiang simplifies events, portraying them in black and white instead of in more subtle shades. Whatever its occasional insights, his volume is too brief and too ideological to supplant the existing literature. It offers far less analysis on the American side than Warren I. Cohen's *American Response to China* (1980), and it offers far less information on the Chinese side than John K. Fairbank's *The United States and China* (1979). Students at any level will learn more about the history of Sino-American relations from either of these books than from this new and tendentious study.

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LLOYD C. GARDNER. *Approaching Vietnam: From World War II through Dienbienphu, 1941-1954*. New York: W. W. Norton. 1988. Pp. 440. \$22.50.

Another book about the origins of American intervention in Indochina? Is there really anything new to be said? Specialists and nonspecialists alike might be forgiven for raising such questions about this latest entry into the "why Vietnam" sweepstakes. After all, within the past several years, Ronald H. Spector, George McT. Kahin, Gabriel Kolko, Gary R. Hess, Andrew J. Rotter, and Melanie Billings-Yun have produced solid and challenging monographs on the subject, all of which overlap in some important respects with this study. Yet a careful examination of Lloyd C. Gardner's latest book should compel even the most skeptical reader to admit that there is indeed much still to be learned about America's early involvement in Indochina, especially the critical period of 1953-54.

In a learned account written with verve, wit, and a keen sense of irony, Gardner locates decisions on Indochina at the intersection of U.S. national security objectives and great power diplomacy. Gardner's main purpose is not to deepen our knowledge of Vietnamese internal developments

or French colonial policy but to explicate "the way American policymakers perceived Vietnam within the outlines of a global vision" (p. 12). In the first one-third of the book, he traces the emergence and evolution of that vision through the Franklin D. Roosevelt and Harry S. Truman administrations. Gardner follows most recent works in emphasizing the growing importance of Indochina to American strategic and economic priorities in the late 1940s. The perceived need to "draw the line" against communist advances somewhere in Southeast Asia in the wake of the Communist Chinese triumph joined with concern about Japanese economic recovery to help transform a colonial struggle into another theater of East-West conflict. The author offers little that is truly original in this section of the study; nonetheless, he provides an exemplary synthesis of recent scholarship that is as sound as it is stylish.

The remaining two-thirds of the book analyzes Washington's response to the multiple challenges that followed the erosion and ultimate collapse of the French position in Indochina in 1953-54. Based on solid research in recently opened American and British archives, it is brimming with new information and provocative interpretations concerning the Bermuda, Berlin, and Geneva conferences, the fate of the "united action" initiative to save Dienbienphu, the origins of the Southeast Asia Treaty Organization, the role of atomic weapons in American strategy and diplomacy, the contentious nature of the Anglo-American relationship, and the centrality of the "liberation" strategy to President Dwight D. Eisenhower's foreign policy. The theme of Eisenhower's focus on "liberation" runs throughout Gardner's treatment of this period. He argues that Eisenhower and Secretary of State John Foster Dulles feared that the consequences of a communist victory in Indochina would be incalculable to America's interests in the region as well as to its world-wide prestige. Consequently, they sought desperately to find alternatives to an Indochina dominated by Ho Chi Minh, convinced that the area must be liberated both from revolutionary nationalism and from anachronistic colonialism. With the fall of Dienbienphu and the subsequent partition of Vietnam at the Geneva Conference, the liberation strategy breathed new life. "Getting rid of the French was the key," Gardner writes. "After that all things were possible" (p. 338).

This study testifies eloquently to the advantages of a multiarchival approach. Many of Gardner's shrewdest insights emerge from his British sources. His narrative captures the competing national interests and the grating personality conflicts that plagued Anglo-American relations during these years. If Americans at times displayed

contempt for Britain's declining power and will and disgust with Prime Minister Winston S. Churchill's fixation on summit diplomacy, the British proved equally resentful of and frightened by the simplicities of American cold war thinking and the dangers of the "massive retaliation" mentality. Gardner probes these differences in arresting detail, showing how they blocked the development of a common Western policy toward Southeast Asia. Indeed, the Anglo-American dimension of the Indochina crisis of the early 1950s, glossed over in previous studies, stands as a signal contribution of this volume. Far more than any previous authority, Gardner demonstrates that London (and Paris) posed nearly as significant a threat to Washington's policy objectives in Asia as did Moscow and Peking.

As complex as they were fateful, America's initial commitments in Vietnam will surely attract many interpreters. Perhaps cultural and intellectual historians can someday shed as much light on U.S. actions in Vietnam as diplomatic historians. This book provides neither the last word on the subject nor the only useful perspective within which to view U.S. decisions or Indochinese developments. The near-exclusive Anglo-American focus of this study may even cloud some important issues as it illumines others. Those caveats aside, Gardner's valuable book commands the attention of all who would understand the roots of America's Vietnam agony.

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ANDREW J. ROTTER. *The Path to Vietnam: Origins of the American Commitment to Southeast Asia*. Ithaca: Cornell University Press. 1987. Pp. xii, 278. \$29.95.

This is an extremely important book. It has long been recognized that, in the aftermath of the reunification of China by Communist forces, the United States embarked on a program of military and economic aid to Southeast Asia during the first half of 1950 and that in the process the first American commitment was made to assist the French in Indochina, despite the absence of any guarantee of Vietnamese independence. It is also well known that during the same period U.S. policies toward Japan underwent a profound change that culminated in the Treaty of Peace with Japan of 1951. And, although it is not an especially original point to make, Great Britain experienced a financial crisis in 1949, which could only be resolved through the devaluation of sterling, while Britain's capacity to earn dollars was closely tied to the sale of Malayan rubber and tin to

the United States. What Andrew J. Rotter has done, very convincingly, is to demonstrate that those three apparently distinct themes in the international history of the period were all part of a single framework of U.S. debate and decision making on a global scale. He does that, moreover, not on the basis of speculative or theoretical analysis but through a careful study of government archives in the United States and Britain. (The French archives, which are now becoming available for the period, were not yet open to scholars at the time he was conducting his research.) He is able to show that American decision makers were well aware of the inter-connectedness both of political and economic considerations and of their policies toward different areas of the world.

The close relationship between Japanese economic recovery and U.S. economic aid to Southeast Asia has already been explored in detail by W. S. Borden in his path-breaking study *The Pacific Alliance* (1984). Rotter takes that exploration further to show that by 1949–50 an analogous relationship was recognized between the economic recovery of both Britain and Western Europe and the economic development of Southeast Asia. That relationship required political stability and the defense of Western interests in the region. Marshall Plan aid, he argues, did not save Europe—at least, not by itself. By 1949 the sterling area again faced a financial crisis, the key to which lay not in Britain's own industrial exports but in the low prices and declining output of Malayan rubber and tin. The Truman administration chose to ignore the demands of domestic producers of synthetic rubber and the Texas smelting industry and to resume stockpiling purchases from Malaya (and eventually also from Indonesia). That decision enabled Malaya to withstand the impact of a Communist-led rebellion that threatened the whole of Western European economic recovery. The economic significance of Southeast Asia for both Europe and Japan also focused the attention of the United States on the critical importance of the struggle in Indochina, which, with Chinese support, had entered a new and more ambitious stage early in 1950. The conclusion that the United States had to support France and its Vietnamese associate Bao Dai was unavoidable, and Rotter clearly points out Britain's interest in securing action by the United States. Rotter's conclusion, in effect, is that the domino theory was first propounded not in Washington but in London.

What is missing from the book, unfortunately, is any examination of Chinese and Soviet perceptions of this complex situation. Marxist-Leninist theoreticians in the late 1940s were unlikely to have ignored the vulnerability of the United States and its allies in Southeast Asia. Rotter is perhaps

too ready to assume that the "Communist threat" was entirely the invention of British and American cold warriors. The very thoroughness of his own research in Western archives, however, serves to highlight the limitations of our knowledge of the debates and decision making of the leading Communist parties involved in the struggle for Southeast Asia, which was just getting under way during 1949–50. Until an equally thorough analysis has been made of such documentation as is available, any interpretation of the motives and calculations of Moscow and Beijing must be provisional. It would, of course, be unreasonable to expect everything from one book. Rotter accomplishes what he has set out to do, and he deserves to be congratulated on his major contribution to what must inevitably be a continuing historical debate.

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MARTIN BEGLINGER. "Containment" im Wandel: Die amerikanische Aussen- und Sicherheitspolitik im Übergang von Truman zu Eisenhower. (Beiträge zur Kolonial- und Überseegeschichte, number 41.) Stuttgart: Franz Steiner. 1988. Pp. 341. DM 58.00.

The year 1953 was a watershed in American and world politics. In January, in the United States, the Democratic party relinquished the presidency that it had held for twenty years, and Joseph Stalin's death in March terminated his twenty-six-year dictatorship in the Soviet Union. While Stalin's successors kept their own counsel behind the Iron Curtain, President Dwight D. Eisenhower and his secretary of state, John Foster Dulles, embarked on a year-long, much-publicized overhaul of U.S. cold war policy, particularly containment, its strategic capstone. The resulting mixed bag of declarations and initiatives—liberation (to diminish the territory under Soviet control), the Captive Peoples resolution, the Korean armistice, the "Chance for Peace," and "Atoms for Peace"—culminated in the "New Look" in national defense and massive retaliation that stood as the twin pillars of national security policy to the end of the decade. These are the components of this study.

Martin Beglinger states that his objectives are "to describe and analyze American foreign and security policy in this transition phase of the Cold War" (p. 9). His focus is on the decision-making process and on the decision makers' perceptions of the situation. Each chapter concludes with a *Fazit*, an analytical balance sheet that undertakes to determine the extent of consonance among the perceptions and decisions and the objective reality. The dominant perceptions, as Beglinger sees

them, were the Eisenhower administration's strictly bipolar, East versus West, view of world politics and its conviction that the Soviet Union was permanently committed to hostility toward the United States and to communist expansion by whatever means came to hand.

The recent publication of the volumes of *Foreign Relations of the United States* for 1952–54 has enabled Beglinger to document the policy-making process more thoroughly than has yet been done on the U.S. side of the Atlantic. That is most evident in the three chapters having to do with Project Solarium, NSC 162/2, and the “New Look.” From those volumes the Eisenhower administration's national security policy, whatever else one may think about it, emerges as the coherent product of a systematic effort. In Project Solarium, three seven-member task forces working independently of each other developed studies of policy alternatives: task force A of containment by conventional means as it had been practiced since 1947, task force B of a variant approach to containment that would allow the United States to respond with means and in places of its own choosing, and task force C of a liberation policy. The task forces submitted their studies (begun in March) to the National Security Council in July 1953, and it, perceiving a long-term Soviet threat as the task forces had, undertook a basic review of national security policy. Eisenhower added a requirement to consider the financial aspect, and conventional containment and liberation thereupon went by the board as too expensive. The resulting paper, NSC 162/2, which Eisenhower endorsed on October 30, 1953, adopted the task force B variant as national policy and declared nuclear weapons to be “as available for use as other munitions” (p. 186). Subsequently, in Dulles's speech on January 12, 1954, to the Council on Foreign Relations (in the writing of which, according to Beglinger, Eisenhower had participated significantly), Dulles made public the “New Look” in national defense and its strategic concomitant, massive retaliation.

Beglinger concludes that massive retaliation as a putative response to communist aggression was an “illusory solution [*Scheinflösung*]” (p. 316). In conjunction with the “New Look,” it placated the fiscal conservatives in the Republican Old Guard and assuaged Eisenhower's fear that continued heavy spending on conventional arms would inevitably bankrupt the country, but it was wholly impracticable as national security policy. Moreover, the perceptions from which it derived blinded the administration to opportunities for inducing change in the Soviet system after Stalin's death. One may ask to what extent these judgments themselves derive from objective reality. Was im-

practicability, for instance, in all respects synonymous with ineffectuality? U.S. nuclear superiority was not an illusion in 1954 and for at least a decade thereafter. And how amenable to change was Soviet policy after Stalin? The response to the Berlin uprising in June 1953 exposed a substantial gap between the post-Stalin rhetoric and practice that widened dangerously later during the Berlin and Cuban missile crises.

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STEPHEN G. RABE. *Eisenhower and Latin America: The Foreign Policy of Anticommunism*. Chapel Hill: University of North Carolina Press. 1988. Pp. 237. Cloth \$29.95, paper \$9.95.

Stephen G. Rabe's solid, workmanlike survey of Dwight D. Eisenhower's Latin American policy is a useful addition to the emergent body of archive-based scholarship on U.S.–Latin American relations during the cold war. The general thrust of Rabe's analysis will startle few readers, for he reiterates the conventional view that, from the start, a “virulent anticommunism” (p. 177) was the central feature of Eisenhower's policy toward Latin America.

Entering office “deeply disturbed” by Central Intelligence Agency (CIA) reports that the Kremlin was exploiting deteriorating conditions in the region and that Guatemala's “Communist infection” was “such as to mark an approaching crisis” (p. 31), Eisenhower moved quickly, bolstering repressive Latin American dictatorships with U.S. military aid, “fawning” (p. 84) over some of the region's most unsavory anticommunist tyrants, and unleashing the CIA to destroy the regime of nationalist reformer Jacobo Arbenz in Guatemala. The anti-Americanism that surfaced during Vice President Richard M. Nixon's turbulent trip to South America in 1958 shattered the administration's “smug confidence” (p. 100) that its hemispheric containment policy was working effectively, forcing a hasty policy adjustment. Soon Eisenhower was cultivating democratic leaders in Latin America and addressing the region's socioeconomic needs for the first time by agreeing to underwrite a regional development bank and relaxing traditional U.S. opposition to international commodity agreements. The Cuban revolution then raised the sudden fear that the United States might lose the cold war in Latin America, prompting a dramatic new policy shift that featured renewed covert intervention to overthrow Fidel Castro together with an “alliance for progress” prototype—a \$500 million Social Progress

Trust Fund to finance socioeconomic reforms and eliminate conditions that might spawn future Castros.

Although the broad contours of that history are already well known, Rabe colors them in with an abundance of interesting details and interpretive nuances that are the book's real strengths. We learn, for example, that in January 1954 Eisenhower warned the Guatemalan foreign minister that the United States was "determined to block the international communist conspiracy" and that Eisenhower "couldn't help a government which was openly playing ball with communists" (p. 47). Just prior to the intervention, Secretary of State John Foster Dulles sought to dispel allegations that the administration's hostility to Arbenz was economically motivated by urging the Justice Department to file an antitrust suit against the United Fruit Company. Shortly after Arbenz's overthrow, his CIA-backed successor, Colonel Carlos Castillo Armas, subserviently informed Nixon: "Tell me what you want me to do and I will do it" (p. 58).

In responding to the Cuban revolution, Eisenhower initially sought to work with Castro, hoping, in Nixon's words, "to orient him in the right direction" (p. 124). Unable to tolerate Castro's nationalism and nonalignment, however, Eisenhower by January 1960 was considering plans to oust the Cuban leader, even though Secretary of State Christian Herter admitted that the United States lacked "hard evidence" (p. 130) that Castro's government was dominated by Communists. Eisenhower then "intentionally provoked" the Cubans into a "new cycle of tension and hostility" (pp. 163–64). Rabe speculates that subsequent U.S. economic warfare and the continuing arms embargo probably "forced the Cubans to seek assistance from the Soviet Union" (p. 132). Herter emerges as, if anything, a more hardbitten cold warrior than Dulles. In January 1961 he contemplated overthrowing Castro by provoking the Cuban leader into some rash act that would provide a pretext for direct U.S. military intervention and suggested to Eisenhower that perhaps "we should stage an 'attack' on Guantánamo" (p. 172). The president, in considering the possibility of severing diplomatic relations with Cuba, commented that "the quicker we do it, the more tempted Castro might be to actually attack Guantánamo," giving the United States an excuse to kick him out "with force" (p. 172). Eisenhower also reportedly expressed his desire to see both Castro and Dominican Republic dictator Rafael Trujillo "sawed off" (p. 157).

Rabe's summary assessment is mixed. He finds Eisenhower a "decisive, energetic, and well-informed" leader who personally directed a "con-

sistent" and "coherent" Latin American policy (p. 174). In the end, however, Rabe views Eisenhower as a practitioner of traditional U.S. sphere-of-influence politics in Latin America—a chief of state who was less than restrained in his use of covert interventionism and whose "fears about Soviet imperialism in the Western Hemisphere were derived more from inference and analogies from other areas of the world than from dispassionate analyses" (p. 40).

As is so frequently the case in works of U.S. diplomatic history, Rabe's study does not fully capture the complexities of policy motivation on the Latin American side of the inter-American equation. The primary thrust of Latin America's diplomatic initiatives toward the United States during the period, he suggests, was a sustained appeal to Eisenhower to assist Latin American economic development by funding a "Marshall Plan for Latin America." Such a characterization, although accurate as far as it goes, ignores the extent to which Latin America's governing elites were deliberately manipulating U.S. anticommunist paranoia throughout the 1950s with ominous and exaggerated reports of a growing Communist menace in their societies as a tactic to obtain the U.S. foreign aid that could finance paternalistic state programs, protect traditional class hierarchies, and line the elites' own pockets.

Nevertheless, Rabe's book treats the U.S. policy side in an impressive and extremely well-documented fashion, and in doing so it is likely to remain the standard reference for some time to come.

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GUENTER LEWY. *Peace and Revolution: The Moral Crisis of American Pacifism*. Grand Rapids, Mich.: William B. Eerdmans. 1988. Pp. x, 283. \$19.95.

In what purports to be a scholarly appraisal of four American pacifist organizations—the American Friends Service Committee (AFSC), the Fellowship of Reconciliation (FOR), the War Resisters League (WRL), and the Women's International League for Peace and Freedom (WILPF)—Guenter Lewy has written an extraordinarily vituperative account of their recent activities. Originally, he argues, these groups favored democracy and nonviolence, but, beginning in 1965, they shifted to ardent backing of Communist military victory in Vietnam and, later, around the world. The AFSC was "part of a network of support for the NLF and Communist North Vietnam" (p. 222). The FOR "has done everything . . . possible to aid . . . Marxist-Leninist movements

in Latin America" (p. 162). The WRL has shown an "infatuation with the mystique of revolution" (p. 163), including Libya's. And the WILPF has become "a Soviet auxiliary" (p. 220). Lewy concludes that pacifists have betrayed themselves and America and that, henceforth, "the proper course" for them "is to remain silent" (p. 248).

Although Lewy draws on organizational records and periodicals, his strident phrasing, hawkish preachments, and misuse of evidence make his case very unconvincing. Advocacy of U.S. withdrawal from Vietnam as well as criticism of corporate capitalism and support for nonviolent civil disobedience are used as measures of Communist intent. Often the writings or actions he cites are those of individuals and do not represent organizational policy. Even when he does deal with official statements, his sweeping, inflammatory summaries do not reflect the nuances in those statements. Another of Lewy's methods is to present opposition to U.S. foreign or military policy as if it came largely, sometimes entirely, from Communist and pacifist sources. In this fashion he reduces even the broadly based nuclear weapons freeze campaign to "yet another tool of the anti-American Left" with "the AFSC again leading the pack" (p. 190).

Pacifist activities that do not fit Lewy's framework are contemptuously dismissed or curiously transformed. The AFSC's condemnation of the Soviet invasion of Afghanistan as "a brutal, immoral, tragic adventure" somehow becomes a "crude piece" of pro-Soviet "propaganda" (pp. 184-85). A strong plea by the FOR for nonviolence by opponents of the Vietnam War is characterized as "a somewhat lame afterthought" (p. 67). Lewy brushes off an article in the FOR's magazine highlighting political persecution in Kampuchea as "a rare instance of candor" (p. 145). When the WRL challenges the state-run Soviet Peace Committee to defend genuine pacifists and other dissidents in the USSR, Lewy uses this action to bolster charges of fellow traveling. Although the AFSC devoted 85 percent of its Vietnam medical aid to areas controlled by the Saigon government, Lewy reports that such aid went exclusively to North Vietnam and the NLF.

This malevolent portrait of American pacifism not only distorts the record but also exacerbates problems of great significance: how to relate to Third World revolutions; how to deal with authoritarian movements and regimes; and how to support peace without betraying freedom. American pacifists have not always chosen the best solutions to these problems, nor have the American people and their government. But they are not helped to

do so by overwrought, reductionist studies that generate more heat than light.

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ROBERT WUTHNOW. *The Restructuring of American Religion: Society and Faith since World War II*. (Studies in Church and State.) Princeton: Princeton University Press. 1988. Pp. xiv, 374. \$25.00.

This book is not for the short of wind or faint of heart, but those readers who stay the course will be generously rewarded. The study is strenuous going because of the subject matter and the nature of the author's sources, not because of the presentation. In fact, Robert Wuthnow, a sociologist, writes with clarity and vigor, consciously concerned "to keep the discussion relatively free of technical jargon and obscure social science language" (p. 323), and he does not employ so much as a single graph or statistical table. The restructuring of American religion is a large subject made even vaster by Wuthnow's correct conviction that the swirling religious scene can be understood only within the context of broader economic, political, cultural, and demographic forces that have dramatically altered the fabric of American life since Hiroshima. Consequently, the book illuminates great reaches of society beyond the confines of cathedral and chapel, tabernacle and temple. The sources tapped are primarily polls, surveys, statistical abstracts, church yearbooks, and of course scholarly articles and monographs rather than unpublished archival materials such as letters, diaries, journals, and introspective meditations. Inescapably, then, the book is inhabited by faceless groups or by individuals identified only by name and not portrayed in their full humanity, the result being a certain bloodlessness. Still, so mountainous is the factual material on such a myriad of subjects and so thoughtful are the author's interpretations, grounded in a mastery of social science theory, that one can only be grateful for this brilliant examination of the public dimensions of religious culture.

Wuthnow discerns major changes in the past half-century, and, while he denies that religion has beat a humiliating retreat in the face of secularization, he also raises disturbing questions about the great transformation. The discussion of the "decline of denominationalism" is outstanding as is the examination of the concurrent, astonishing rise of "special purpose religious associations." Wuthnow convincingly roots both phenomena in new patterns in education, employment, mobility, race and gender relations, and the role of govern-

ment. We learn much about the immediate post-war period of "promise and peril" and the decline of organized religion in the shattering 1960s. The analysis of the rise of religious conservatism is especially insightful because the author properly draws a distinction between the older evangelicals such as Billy Graham and Carl F. H. Henry and the National Association of Evangelicals (NAE) and the New Christian Right associated with Jerry Falwell, Jim Bakker, Jimmy Swaggart, and company. Also thoughtful are the commentaries on civil religion, technology, feminism and ordination, and, finally, religious symbolism.

Splendid as this study is, it is not quite inerrant. Doubtless it is a matter of training and temperament, but my distrust of what polls and surveys reveal is sharper than Wuthnow's. The section on Protestant-Catholic tensions lacks concreteness and fails to capture the intense fears of the liberal Protestant establishment. No mention at all is made of the Second Assembly of the World Council of Churches in Evanston in 1954, a signal event in non-Roman Christianity in America. Existentialism is not given a fair hearing. Prewar political activity by the churches is underestimated (p. 65). The Methodist church did not, either in its theological or in its social positions, "in fact remain strongly identified with a relatively conservative strand of the Protestant tradition" (p. 142). As Wuthnow surely knows, by the time of the cold war, Reinhold Niebuhr was no longer identified with the "religious left" as is implied (p. 143). Legislation has not "virtually cut off immigration since the 1920s" (p. 278). While the notes are full and informing, the bibliography is merely serviceable, and the index is a disaster.

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DENNIS SMITH. *The Chicago School: A Liberal Critique of Capitalism*. (Theoretical Traditions in the Social Sciences.) New York: St. Martin's. 1988. Pp. x, 245. Cloth \$45.00, paper \$14.95.

The development of a tradition of sociology, in particular, "the Chicago School," which Dennis Smith defines by ideological commitment and political goals, is the object of this volume. The Chicago School refers to a diverse range of sociologists—from Albion Small to Robert Park and Louis Wirth to Morris Janowitz and others in between—connected to one degree or another with the sociology department of the University of Chicago from the late nineteenth century to the present. What makes them a school in Smith's recounting is their common commitment to

American liberal values and institutions and to the constant struggle they believed they faced in an environment that seemed to them consistently hostile to those determined to consider the common good and look beyond immediate self-interest. Thus, Smith's Chicago School shares no single methodology, research project, or conception of what sociology is, in contrast to so many other "schools" of sociology, past and present. Its very American reform liberalism has been its modest (and, it should be clear, not especially theoretically oriented) faith.

Given his interpretation, Smith tells his story well. He treats the Chicago School he identifies and its principal figures succinctly, on their own terms and comparatively. He sets each in historical context and in the context of the school's history. He pays attention to the argument and writing of each social scientist but does not neglect the relevant factors of personal background and life story. Smith also provides some good points of comparison, especially in his nice chapter on Thorstein Veblen and John Dewey.

One might plead for a bit more of Smith's thesis that there really was a Chicago school or tradition and, in the process, look for a richer sense of his thinkers' own articulation of their goals. I think a firmer brief might also be made for the importance of the Chicago School. If Smith's view is that the Chicago School has mattered in American intellectual life and history in the twentieth century, his case could be expanded.

As a source for the ideas and accomplishments of "the Chicago tradition" of sociology, one that goes well beyond listing facts and crafts a case for its subjects' underlying unity, this is a valuable work, easy to recommend.

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JAMES D. SAVAGE. *Balanced Budgets and American Politics*. Ithaca: Cornell University Press. 1988. Pp. xiii, 350. \$27.95.

Taxing and spending by government at all levels is the very substance of politics. But James D. Savage maintains that only in the United States at the national level has political significance been attached to a very special relationship between those two functions, that is, to receipts equaling expenditures, or the balanced-budget principle. The aim of this book is to trace the source or sources of the budget-balancing principle in American political history.

The idea of a balanced budget as an accounting concept is quite simple: current receipts equal

current expenditures. There is little that is controversial about this definition of a balanced budget. When we transform, however, the descriptive statement to a normative one—current receipts ought to equal current expenditures—we stir up political and economic turmoil. It is this transformation and its origins in the political sphere that the author attempts to explore in his book.

The political as well as the economic appeal of the balanced-budget principle must reside in the alleged deleterious effects of its converse, the unbalanced budget. Unbalanced budgets, as we all know, are feasible. The options to financing an unbalanced budget are well understood: the issue of government debt obligations and the creation of paper money by the state or both. If we take exception to unbalanced budgets (deficit finance), we may do so on the grounds that increases in the national debt are harmful: resort to the printing press is inflationary; deficit finance has negligible effects on total output; there is unwarranted expansion of the range of federal government activity at the expense of state and local autonomy; and there may be an unacceptable redistribution of income.

The politics of the balanced-budget principle arise when political agents or parties form opinions about these anticipated effects, harmful or otherwise. The opinions are usually well articulated, highly simplified, and covered with a good gloss of old-fashioned rhetoric. It matters little if these opinions are inconsistent with the facts; that they be widely held is sufficient. Ignorance as well as knowledge may equally guide public opinion.

But Savage contends that the idea of a balanced budget came to symbolize much more than a simple accounting concept; it came to stand for a particular way of viewing the role of centralized versus decentralized government—that is, limited central government power, states' rights, and prevention of "corruption" through constriction of the influence of "moneyed" interests (bankers, industrialists). This broader view obfuscates the issue. It is not balanced budgets per se to which there is emotional and intellectual commitment. The commitment is to small budgets as opposed to large budgets; minimum expenditures, minimum taxes. The application of the term "balanced-budget principle" misleads.

The origins of the balanced-budget principle are to be found in the colonial experience with paper money; in the Jefferson-Jackson view that unbalanced budgets threatened individual liberty; in the Republican party's commitment over the period 1881–1982 to administrative efficiency, the protective tariff, and the expansion of government expenditures; in the emergence of deficits during the New Deal; and in the unsuccessful

attempt of Ronald Reagan to cope with unbalanced budgets. A chapter is devoted to each of these five phases in the evolution of the balanced-budget principle.

The author has made a courageous attempt to deal with an important problem on an extensive historical stage; it may have been too grandiose a conceptual scheme to carry out effectively. So much is historical interpretation, so little is bricks-and-mortar history. It is difficult to make sure whether the author has his facts correct. One example of his defective historical scholarship may suffice. He has seriously misinterpreted part of the colonial experience with paper money, especially after 1751. No war in American history was financed as successfully as the Seven Years' War. No other imposed such harsh taxation burdens. And its success can be attributed to the issue of self-liquidating paper money ultimately redeemed out of the proceeds of future taxes. Paper money got a bad rap from the pre-1751 experience. And this incorrect interpretation continues to be perpetuated.

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PETER TEMIN. *The Fall of the Bell System: A Study in Prices and Politics*. Assisted by LOUIS GALAMBOS. New York: Cambridge University Press. 1987. Pp. xviii, 378. \$27.95.

On January 1, 1984, the American Telephone and Telegraph Company (AT&T) was split into several pieces. AT&T continued to offer long-distance service and kept Western Electric and Bell Laboratories. The now-independent regional operating companies, or "Baby Bells," provide local operating service. This breakup of the largest privately owned company in the world (it had one million employees) resulted from a prolonged antitrust suit, and, like other great judicial proceedings in American history, the case provides a window on the values and ideologies of its time.

The perspective of this book is mainly from inside the offices of AT&T chairmen John D. deButts (1972–79) and Charles L. Brown (1979–86). DeButts stood like a stone wall against encroaching competition and a flurry of attacks from diverse sets of regulators. He powerfully reasserted the "natural monopoly" vision of "one system and universal service" articulated long before by AT&T's first chairman, Theodore Vail. Brown, however, saw ominous handwriting on many different walls. After a series of upheavals involving the company and its manifold regulators, Brown, advised by the brilliant attorney Howard Trienens, took bold action: he settled the antitrust suit

in one dramatic stroke. He took this "biggest single decision that an American businessman has had to make in the last century" (pp. 267–68) not in a vacuum but as the least unpalatable choice before him. The alternatives included a very unfavorable outcome in the antitrust suit, severe anti-AT&T legislation then before Congress, a continuation of contradictory policies by the Federal Communications Commission (FCC), and a welter of private antitrust actions brought by such companies as MCI and Sprint.

Peter Temin makes clear that the context of Brown's decision cannot be understood primarily on technological, economic, or business grounds. All were important, but the chief driving force was ideology. Americans, Temin argues, wanted two incompatible results. On the one hand, they desired efficient service. When they picked up the receiver, they wanted to hear a dial tone. When they dialed a number, they expected the phone at the other end to ring. Yet, on the other hand, Americans wanted equal entrepreneurial opportunity for upstarts who were trying to compete against AT&T. With their historic suspicion of bigness, Americans simply distrusted any institution as large as Ma Bell. In this sense it did not matter how well the company was doing its job, and Temin shows that AT&T was in fact performing better than any system in any other country. The breakup, therefore, may have been a colossal error of public policy.

What thickened the plot beyond rational resolution was the confusing issue of "cross-subsidization." For many years, AT&T attackers had alleged that the company was overcharging small customers while undercharging businesses and that AT&T was using its monopoly of local telephone service to subsidize long-distance callers. Yet in truth the cross-subsidy was going in exactly the opposite direction. Long-distance lines subsidized local service. Businesses, in general, were subsidizing home users. (Any political scientist could have predicted this; residential users have more votes than do business customers.) Yet the company had set the trap for itself. As Temin does not make sufficiently clear, Bell had traditionally refused to "unbundle" the accounting data it submitted to regulatory authorities. The company liked to keep its charges bundled to maximize its own latitude in pricing decisions and overall corporate management. Thus, over time, the cost of a particular service bore less and less relation to its price, and Bell's credibility began to decline. In regulatory decisions during the 1960s and 1970s, Bell found itself in an exceedingly awkward position.

In the end-game of 1982, the chief mover and shaker was William Baxter, assistant attorney gen-

eral for antitrust cases in the Department of Justice. A Stanford law professor steeped in neoclassical economics, Baxter saw a simple solution: a clean separation between the monopolistic and competitive aspects of Bell's myriad lines of business. The competitive portions—long-distance service, equipment manufacturing, and research and development—would be split off from the regulated local and regional operating companies. After tortuous negotiations, Baxter and Brown came to a settlement that ended the antitrust suit and began the great divestiture of 1984.

Had everything turned out as Baxter and Brown intended, all might have been well. The reorganization within Bell still would have been traumatic, but at least the destination would be clear—a bright line between regulation and competition. In Temin's view, however, the economically sophisticated Baxter had been politically naive, failing to allow for the crazy-quilt pattern of American government. In actual practice, no bright line materialized, and the boundary between regulation and competition remained blurred. The FCC refused to bow out. Judge Harold Greene, a major player because he presided over the antitrust suit, insisted on a continuing administrative role for himself. Congress refused to withdraw, because immense sums of money were at stake (Temin estimates the annual cross-subsidy at \$7 billion) and because telephones remained important in every district in America. Temin underscores that this study is a history of political economy and more of politics than of economics.

His book makes heavy demands on the reader, but overall it represents an outstanding fusion of different kinds of histories: legal, economic, political, business, even cultural. This is an epic story, extremely well researched and documented. Its tragic hero is Brown, the man who commissioned the study. Yet such is the manifest care and scholarly integrity of both Temin and Louis Galambos that we may readily accept Temin's prefatory comment: "I would like to believe that this is the same book I would have written had it been supported by an independent agency. The views expressed here are my own."

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CANADA

GRAHAM S. LOWE. *Women in the Administrative Revolution: The Feminization of Clerical Work*. Buffalo, N.Y.: University of Toronto Press. 1987. Pp. xiii, 234. Cloth \$32.50, paper \$16.95.

In the 1980s office work is widely recognized as a classic ghetto for women workers, defined by

relatively low wages, routine work, high turnover, poor opportunities for advancement, and a high concentration of female employees. The nineteenth-century office, however, was an almost exclusively male domain in which the female office clerks, bank tellers, and civil servants of today were virtually unknown. In Canada, as in Britain and the United States, the decisive transformation took place in the early decades of the twentieth century, and this dramatic restructuring of the work force along lines of gender is the subject of Graham S. Lowe's illuminating study.

His central theme is that the recruitment of increasingly large numbers of female employees must be linked to changes in the structure of the economy at the turn of the century. The period from the 1890s to the 1920s is regarded as a distinct phase in the development of Canadian capitalism, when regional enterprises expanded on a national scale and the American branch plant became a significant presence. The rise of big business and big government entailed a large-scale "revolution in the means of administration" (p. 8) designed to enhance the possibilities for coordination and control of economic and public life. Large numbers of new employees were required to power the operations of the modern office, and the result was a dramatic increase in the clerical sector of employment. As part of this revolution, new technologies brought the office out of the realm of the workshop and closer to the factory, although it was only in a few areas such as the typing pools and keypunch operations that full factory conditions prevailed. Meanwhile, the principles of scientific management were also being introduced, and the 1920s in particular witnessed an intensive round of efficiency controls and rationalization in office procedures.

It was in the course of this revolution that office work was redefined as low-wage female work. As the modern office took shape, women provided the necessary hands, causing sharp increases in the numbers of women workers and in the rate of women's participation in the labor force. For the purposes of the office revolution, educated young women represented a large pool of labor with appropriate skills to meet the requirements of the changing administrative workplace. Technologies such as the typewriter, which were originally gender-neutral, became firmly associated with this new world of women's work. From this perspective it was not inevitable that the work force would become predominantly female. Once the process of female recruitment was set in motion, however, the redefinition of office work was swiftly ratified by patriarchal ideologies, and the sex-labeling of women's work has remained a durable force to the present day. Consistent with his analysis, Lowe

concludes that barring structural changes in the economy or effective challenges to patriarchy, the segmentation of the female labor market is likely to remain in place.

These are large structural considerations, of course, but this is sociology with a strong empirical flavor, and the author offers insights into a number of the contingent decisions that resulted in the creation of a separate female labor market. Using business journals and the records of individual firms, Lowe is able to reconstruct the entry of women into the key clerical occupations in Canada. It is clear that individual firms moved at different speeds in this administrative revolution, and considerable evidence is presented regarding changes in office management and the introduction of new technology. The case of the banks is particularly interesting. Major enterprises such as the Bank of Nova Scotia were in the forefront of the administrative revolution, and the same centralized accounting methods that governed the flow of capital in the branch banking system also inspired a standardized efficiency rating system for the evaluation of bank clerks. Nevertheless, the banks were relatively slow to appreciate the opportunities to exploit female labor, and it was only the labor shortages of the First World War that forced them to abandon longstanding prejudices against female tellers, with rapid and lasting results.

Like other scholars, Lowe uses the convenient term "feminization" to describe the process. But he makes it clear that the notion that female workers "replaced" male workers is misleading, for women were entering new jobs created by the administrative revolution. The term "feminization" more aptly refers to the creation of a separate and unequal female labor market in which menial work, close supervision, high turnover rates, and low expectations were considered to be the norms. Job ladders for advancement reached down into the male segment of the market, but women's office work was seen not as preparation for a business career but as a temporary phase in a female work cycle focused on family and home. This new labor market was most rigidly defined in the public service, where regulations restricted women to junior classifications and the hiring of married women was officially prohibited.

This study is a persuasive one, distinguished by a broad approach to theoretical issues and by keen attention to comparisons with British and American developments. It is, however, a work of historical sociology, not of social history. One is reminded of E. P. Thompson's famous dictum that the working class made itself as much as it was made, and it is clear that much remains to be said

about the experience of office workers. Lowe recognizes the difficulties in defining women office workers simply as members of a new proletariat and agrees that they must be more firmly situated in the context of women's history. Employment records should cast light on issues such as recruitment patterns and rates of turnover, levels of skill and varieties of discipline, and the ethnographic evidence that may still be gathered from veterans of the first generation of female office workers should tell us much about how women responded to the administrative revolution. The relationship of women's work to family histories and household patterns requires exploration, especially if we are to understand how office work displaced the existing female ghetto of the time, namely, domestic service. And, if the appeal of clerical unionism was limited in this period, there is undoubtedly a detailed history of adaptation and resistance to be told in this workplace as in others. In short, this book is not the full account of how office workers "lived the big changes" of the twentieth century, but Lowe has given us the beginning of an explanation, and his contribution should be recognized as a study of international interest.

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LATIN AMERICA

INGA CLENDINNEN. *Ambivalent Conquests: Maya and Spaniard in Yucatán, 1517–1570*. (Cambridge Latin American Studies, number 61.) New York: Cambridge University Press. 1987. Pp. xiii, 245. \$34.50.

This is a splendid book by a gifted historian. With great subtlety and imagination, Inga Clendinnen draws us into the swirl of missteps, ambitions, deceptions, and fantasies that constituted the conquest drama in Yucatán. In this frontier peninsula, distant from the glamour and riches of central Mexico, the power of the early Franciscan missionaries, bent on molding the Maya into willing beneficiaries of Christian persuasion, eclipsed that of Spanish explorers and settlers moved by more worldly drives. The native Maya appear in a variety of stances—alternately defiant, compliant, victimized, adaptive. Amid this diversity, one senses a unifying leitmotif, subtly brought into focus by Clendinnen: the Mayan attempt to garner a measure of autonomy and tranquillity in a destructive era by nurturing a sense of independent identity and of continued access to the secrets of high knowledge (including Christian knowledge) that assured the predictability and continuity of life.

The climactic episode in the Franciscan-Maya

encounter was the double scandal of 1562: first, the discovery that the Maya, even the most trusted Christianized schoolmasters on whom the missionaries had lavished their greatest efforts, secretly practiced blasphemous idolatry, including human sacrifice, in their communities; then, the added shock when the Franciscans reacted to the news not with the humble and self-afflicting forms of persuasion that defined their proclaimed self-image but with uncontrolled rage and cruel violence that terrorized the Maya and broke the legal prescriptions that carefully regulated torture and judicial inquest. It is the crisis of 1562 and the sources it produced that define the core of Clendinnen's inquiry. To this inquiry Clendinnen brings extraordinary sensitivity to the mental worlds of the various actors, including the Maya, and bold insistence on drawing inferences that press us to the most interior reading possible of the historical minds and sources in question.

The author's most important accomplishment, among several, is her simultaneously sympathetic and hard-hitting analysis of the Franciscan missionaries and their leader, Diego de Landa. She argues that the Franciscans' cruelty derived from the rage of betrayed paternalism. The mix of self-delusion and self-denial by missionaries intent on seeing themselves as selfless guardians of their Christian novitiates proved volatile once the self-delusion became apparent. The "betrayal" of the Indian was too cruel a blow, and the unbounded violence of the missionaries exposed "something of the emotion-charged punitive rage of the betrayed parent" (p. 113). Curiously, the European background to the Franciscans' self-image, the values and experience of self-abnegation and humility in the Mexican missionary effort, and the transformation of the Franciscans' paternal sacrifices on behalf of Indian innocents into furious vengeance against those who "betrayed" the missionaries' trust come through somewhat less powerfully in this book than in Clendinnen's brilliant article on missionary violence ("Disciplining the Indians: Franciscan Ideology and Missionary Violence in Sixteenth-Century Yucatán," *Past and Present* 94 [1982]: 27–48). And it is not entirely clear how to integrate, in the explanation of the Franciscans' behavior in 1562, the self-abnegating side of the Franciscan experience with the author's masterful portrait of Landa's ruthless ambition, power-seeking personality, and adroit political manipulations. (Such an integration is in my view quite possible, but the author is overly circumspect on this point.) Nonetheless, to read Clendinnen's book and 1982 article in conjunction is to read the most striking and revealing scholarship about the early Mexican Franciscans in decades.

I wish to record one quibble that affects the

overall assessment of the period. It is a commonplace to refer to colonial Yucatán as a stagnant backwater and therefore to view early colonial economic pressures as comparatively weak or benign. Clendinnen adheres to this view, which appears accurate for much of the mid- and late colonial periods. In doing so, she perhaps underestimates the economic forces both the Franciscans and Maya were up against in the early period under study, when Spanish settlers were eager to develop substantial regional export trades in cotton textiles, honey and wax, and indigo, when the transport of goods relied more on Indian porters than on horses and mules, and when Central American competition had not yet ruined the Yucatecan indigo trade. None of this makes Clendinnen "wrong," but it somewhat alters the total context that framed the Maya peoples' ambivalent receptivity to the Franciscans and the rivalry between the Franciscans and their more worldly compatriots. The familiar issues of economic abuse and settler anxiety that missionaries would restrict too sharply the settlers' control of tribute and labor may have figured significantly in the story, even in Yucatán.

Clendinnen has written a remarkably powerful and compelling book. Next to this achievement, the quibbles and caveats are minor. Scholarship on Yucatán has enjoyed both a distinguished tradition reaching back to the 1930s and a remarkable boom in the 1980s. This study ranks among the very best scholarship on the region and will dazzle any serious student of native American peoples, Christian missionaries, and colonial situations.

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ROSS HASSIG. *Aztec Warfare: Imperial Expansion and Political Control*. (Civilization of the American Indian Series, number 188.) Norman: University of Oklahoma Press. 1988. Pp. xx, 404. \$29.95.

This is a good and sorely needed book in the field of Aztec studies. Ross Hassig's subject is the particular style of warfare practiced by Tenochtitlán. Properly he reveals for us his methodology, the difficulties his sources always occasion, the chronological problems of assigning years to the subjugation of the enemy and allied cities, and so forth; curiously, however, he tucks these useful remarks away in an obscure footnote (chapter 9, n. 5). Following Edward Luttwack, he carefully distinguishes force from power, and it is from this understanding that he derives the hegemonic character of the Mexican state. He is also acquainted with John Keegan's remarkable *Face of*

Battle (1983). As an indispensable aid to the clarification of his work, Hassig provides twenty-one maps that define all of the campaigns of the individual Mexican rulers; these maps are carefully woven into the text. The book is divided into two parts. The first is a description of Aztec warfare (tactics, weaponry, mobilization, encampment, combat). The second is the story of this form of warfare in the Mesoamerican chronology from Acamapichtli to Cuauhtémoc. Hassig believes Aztec warfare to have been more pragmatic than has been previously thought. He does not in any sense deny the religious aspects of Aztec warfare, but he feels that religion has been over-emphasized in the past: "religious motivations were the handmaidens of empire and not its impetus" (p. 266). Finally, it is good to report that he is competent in Nahuatl and is in addition a dependable proofreader (a daunting matter for a book abounding in Nahuatl names).

With a book such as this, every interested reader is bound to have certain quarrels. I have several, but they in no sense prejudice the value of the work. In one area particularly I believe the picture he draws is inadequate, that is, his discussion of the *xochiyaoyotl*. He sees "the flower war" as merely a strategic step in the progress of Mexican arms, thus characterizing it as more secular than ritualistic. The *xochiyaoyotl*, unlike the state-supported battles generally fought to subject the enemy, was rather a radical and exotic attempt to maintain the nobles' monopoly in status by powerfully satisfying the thirst of the gods for blood.

In one other regard Hassig can be brought under scrutiny, namely, in the picture he paints of the underlying causes of the Spaniards' victory over the Mexicans. Here he follows Luttwack, for he sees as fundamental to the Spanish success their lack of participation in the Aztec understanding of how power was to be apprehended. The Spaniards had had no previous encounters with the Aztecs and thus were not initially put off by a knowledge of their previous successes. This happy ignorance was the key to the Spanish victory according to Hassig. In other words, the Spaniards did not know how the Aztecs played the power game; thus, the Spaniards changed the rules. They assessed the Mexicans in terms of the force they could bring to bear on the field of battle and not in terms of an indefinite threat of force. As for the Aztecs, Hassig admits he has no good explanation of Moctezuma's extraordinary welcome of the Spaniards and their Tlaxcalteca allies into Tenochtitlán.

The book will undoubtedly create a real scholarly interest. It belongs in the library of every

student of Mesoamerica. It should also be a must for the military historian.

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BRIAN L. MOORE. *Race, Power, and Social Segmentation in Colonial Society: Guyana after Slavery, 1838–1891*. (Caribbean Studies, number 4.) New York: Gordon and Breach Science. 1987. Pp. xiv, 310. \$38.00.

Brian L. Moore's meticulous examination of colonial records, plantation documents, church correspondence, and period newspapers from abolition to the disturbances of 1887–89 and ensuing reforms produces an invaluable picture of nineteenth-century Guyana. Moreover, he makes good his claim that "social science theory can be successfully applied to a corpus of historical data with beneficial results" (p. 223)—and without becoming unreadable.

Moore's goal is to resolve the dispute between conflict- and integration-oriented models of the ethnically plural society and, more specifically, those authors who apply such models to the Guyanese case. In these two categories, Moore scrutinizes, among others, the work of M. G. Smith, Harmannus Hoetink, John Rex, Pierre van den Berghe, R. A. J. van Lier, and Raymond T. Smith, Lloyd Braithwaite, George Beckford, Walter Rodney, and Leo A. Despres, respectively.

His methodology is exemplary; he finds ways to examine the changing salience of class, race, and culture in the various conflict situations in the colony. He finds, for example, that incorporation of coloreds into the power structure by the 1890s may express a limited political and cultural integration with whites without reducing the class or culturally plural nature of the system (that is, without resolving the theoretical issue one way or the other). Similarly, he finds Rodney's belief in the "indigenizing experience" of the plantation insufficient to render Creoles and East Indians potential allies, given the negative racial perceptions that were also basic to that experience.

White manipulations of suffrage, land, and commercial law as well as public works policy provide incontestable proof of the class nature of government. Assemblage of such a multicultural population (through the nineteenth century, including immigration of Portuguese, Chinese, and Indians) clearly permitted the elite to divide and conquer among both the lower-class level of black ex-slaves and other non-whites and the emergent middle class of coloreds, Portuguese, and Chinese. But Moore finds it too easy to categorize the

accompanying racial prejudice as externally imposed. Although he does not cite statistics, he finds "few" cross-cultural marriages except between whites and Creoles and few examples of common cause except among Portuguese and coloreds.

Because some integration occurred, he concludes that "neither the theories of stratification nor of pluralism are sufficient to explain the changes which composite societies undergo over a significant time period" (p. 220). But, because his portrait of Guyana falls somewhere between the two as an ever-changing complex hybrid, he insists that the theories may in fact be compatible in a way that is still beyond our grasp.

My only reservation about this otherwise flawless exercise is the author's use of race as the larger ethnic category in his theorizing vis-à-vis class. In the body of his text, he uses race, language, culture, and religion as coequal variables. The larger category, encompassing all four, should be ethnicity.

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ELIZABETH MCLEAN PETRAS. *Jamaican Labor Migration: White Capital and Black Labor, 1850–1930*. (Westview Special Studies on Latin America and the Caribbean.) Boulder, Colo.: Westview. 1988. Pp. x, 297. \$31.50.

Jamaica, like most of the nations of the Caribbean region, was a voracious consumer of immigrants for the long period from the voyage to the Americas by Columbus to the middle of the nineteenth century. But, with the growth of the population through natural increase and the decline of the sugar industry following emancipation, the island became a net exporter of people. Between 1850 and 1930 most of the migrants sought employment within the region. Manual labor on the Panama railroad and the French and U.S. canals accounted for the majority of jobs. Work on Cuban sugar plantations became important in the early twentieth century, after the completion of the canal, but the 1920s saw a closure of opportunities and a significant return migration.

In this study Elizabeth McLean Petras offers the first general account of the specifically Jamaican components of these migrant flows. She views the movement as part of the international division of labor, an inevitable consequence of the development of global capitalism, but also identifies features of Jamaican economic development that help explain the character of the island's involvement. Of the factors contributing to Jamaican economic growth, Petras argues that the most important was the long-term continuity of a com-

bined form of peasant-proletarian relations with its roots in the provision grounds system that created a subeconomy of market production during the period of slavery. This type of production, together with the arrest of proletarianization at an intermediate stage, resulted in the geographical splitting of households. Males migrated from the periphery to participate in core production, in a series of locations, leaving behind females and the young and elderly who maintained an engagement in household subsistence agriculture.

The interpretation of the pattern of Jamaican labor migration offered by Petras is the most original feature of her work. The book comes in the wake of a series of recently published studies with which it overlaps. The most important of these are Velma Newton's *The Silver Men: West Indian Labour Migration to Panama, 1850–1914* (1984), Michael L. Conniff's *Black Labor on a White Canal: Panama, 1904–1981* (1985), Bonham C. Richardson's *Panama Money in Barbados, 1900–1920* (1985), and Franklin W. Knight's essay "Jamaican Migrants and the Cuban Sugar Industry, 1900–1934," in *Between Slavery and Free Labor*, edited by Manuel Moreno Fraginals, Frank Moya Pons, and Stanley Engerman (1985). Petras mentions none of these contributions but uses Newton's master's thesis (1973) very heavily (and incorrectly cites it in the text as completed in 1977). The amount of research carried out by Petras in primary documents is slight. What she offers is new insofar as it provides a perspective specific to Jamaica. There are problems here, however, particularly because Jamaicans are difficult to distinguish in the historical record during the important period of U.S. canal building. The tendency is to assume West Indian means Jamaican, and the ways in which the Jamaican experience differed from that of Barbados are not investigated adequately. Some chapters contain very little on Jamaica and serve merely to repeat information readily available elsewhere. The book will be read more for its theoretical arguments than for its contribution to the identification of regional differences in the pattern of labor migration and its impact.

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VICTOR BULMER-THOMAS. *The Political Economy of Central America since 1920*. (Cambridge Latin American Studies, number 63.) New York: Cambridge University Press. 1987. Pp. xxiii, 416. Cloth \$49.50, paper \$16.95.

This book is the first serious attempt at a comprehensive economic history of twentieth-century Central America. Notwithstanding some shortcomings, it is a major contribution toward a better understanding of the dynamics of the political and economic forces that have made Central America the focus of worldwide attention during the crises of the present decade.

The liberal-capitalist modernization model, which has strongly influenced the Central American economy since the late eighteenth century and has dominated it during the last century, is described by Victor Bulmer-Thomas during the period of its zenith and through the series of crises that have marked its application since 1920. Control of policy by the export sector, to the exclusion of adequate social modification of the model on behalf of the majority of the population, has prevented more general prosperity and made those crises successively more serious. Although the precise nature and extent of the failure of the model varies from state to state, there is a pattern throughout the region, and, as the labor force has become increasingly proletarianized by capitalist modernization, the crises have worsened. Bulmer-Thomas did not invent that thesis, but his detailed statistical support of it makes its appeal ever more irresistible. He effectively documents the relationship between export-oriented economic development and domestic food production, which suffered generally from the 1920s on and contributed to expanded poverty as wages failed to keep up with the rising prices of imported food. Agricultural exports continually required the use of land and labor formerly dedicated to subsistence, forcing rural workers off their land into wage labor that did not provide adequate standards of living.

Bulmer-Thomas has apparently ignored much of the research in Central American history during the last quarter century, resulting in a superficial historical background that contains some annoying, if relatively insignificant, factual errors. That shortcoming is especially evident in his summary treatment of the nineteenth century, which is based largely on older liberal accounts and a few more modern, theoretical works, but, even for the twentieth century, there are surprising omissions from his bibliography.

Since his research in secondary works is less than satisfactory, Bulmer-Thomas's effective presentation of government statistics as the primary source for his work is the volume's principal value. Admitting the risks of reliance on such statistics, which are often inaccurate, deliberately falsified, or incomplete, Bulmer-Thomas has painstakingly adjusted them to provide an apparently sound basis for his study. He explains how he has done that in a methodological appendix. In providing

such a statistical base for much of the twentieth century, Bulmer-Thomas has greatly advanced the study of Central American economic history and has produced a work that will likely be a standard reference for decades to come, much as Dana Munro's *Five Republics* (1918) was in the early twentieth century. In addition to numerous charts and tables in the text, a statistical appendix provides fifteen general tables on gross domestic product, population, production, and trade that support the author's conclusions.

Bulmer-Thomas is an economist, and a rudimentary knowledge of economics is helpful to understanding his work. Organized chronologically and keyed to political change, his political analysis is often superficial or simplistic and could have been improved by greater attention to the historical and anthropological research of the past two decades. Yet he does a good job of explaining the organization and shortcomings of the Central American Common Market and the relationship between economic and political crises in the 1970s and early 1980s. Paying considerable attention to labor in the region, Bulmer-Thomas attributes the development of civil war in Guatemala, El Salvador, and Nicaragua to a failure in those states to accommodate labor's demands for a larger share of the rising GNP, in contrast to a more benign policy toward labor in Costa Rica and Honduras.

He also demonstrates the role of the foreign debt in the growing crisis in each state.

Bulmer-Thomas's research confirms what many others have long said: there is both unity and diversity in the Central American experience. Bulmer-Thomas notes that political crises since 1979 have shifted "the balance between conformity and diversity in the region significantly in favour of the latter" (p. 267). But his work makes it overwhelmingly clear that the export-led model is the dominant feature in the Central American political economies, as he argues that "the theory of export-led growth is based on a number of assumptions, which have not been generally applicable to Central America" (p. 267). That conclusion is elaborated in his concluding discussion of regional macroeconomic performance in the twentieth century and in his analyses of the export-led model through five phases, reform and the labor movement, and external influence, economic policy, and political change. In a final section on the 1979 crisis in historical perspective, Bulmer-Thomas concludes that the Sandinista revolution represents the culmination of the failure of capitalist modernization in Central America and the end of U.S. hegemony in the region.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

LYNN HUNT, editor. *The New Cultural History*. (Studies on the History of Society and Culture.) Berkeley and Los Angeles: University of California Press. 1989. Pp. ix, 244.

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REVIEWS OF BOOKS

TO THE EDITOR:

A couple of comments concerning Eugen Weber's stimulating review of *Pétain* by Marc Ferro (*AHR*, 94 [June 1989]: 787). First, a very small point: the German Marshal was Rundstedt, not Runstedt.

The review begins: "In 1940, the French spent thirty-nine days fighting the Germans before they laid down their arms. They have spent more than a half-century since then fighting each other about it." More? By my calculation, the fiftieth anniversary of the Armistice will only come in June 1990, and the *AHR* is dated June 1989. More important, one might object to "The French," "they," and "They." After all, as early as June 1940, there were some French people who did not lay down their arms. The hundredth birthday of Charles de Gaulle, "l'homme du 18 juin," will be commemorated in November 1990. Correctly or incorrectly, the French Foreign Office has come to consider the Vichy régime as illegal. Jean Lacouture, at the very end of the first of his three-volume biography of de Gaulle, refers to de Gaulle's march down the Champs Elysées in August 1944 as *Le Sacré*. Maybe Pétain was a second Henry VI (crowned at Notre-Dame as king of France) and de Gaulle a second *dauphin*, once mocked as king of Bourges but eventually crowned as King Charles VII of France in Reims. In World War II, some Frenchmen did not lay down their arms until Germany's unconditional surrender in May 1945

to the four major allies: UK, USSR, USA . . . and France.

BERNARD SINSHEIMER

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Eugen Weber does not wish to reply.

THE EDITOR

TO THE EDITOR:

One of the more glaring anomalies of the world of scholarly discourse is that, while a book cannot be published until someone with knowledge of the subject of which it treats attests that it is (at the least) adequately supported and argued, no procedures exist to ensure that a review of that same book will also meet minimum intellectual standards. A reviewer may characterize the book entrusted to him or her in the most groundless fashion, and no qualified observer (that is, no other reader of the book under review) is at hand to place the reviewer under prior restraint. Thus a contemporary author may be as much at the mercy of a reviewer's "hide-bound humor which he calls his judgement" as, according to John Milton, his seventeenth-century predecessor was apt to be at the mercy of some similarly incompetent enforcer of book-licensing legislation. Happily, the situation is mitigated somewhat (and Milton is among those we have to thank for this) by the fact of multiple reviews.

These reflections are occasioned by Mason I. Lowance, Jr.'s remarkably careless and high-handed review (*AHR*, 94 [June 1989]: 840–41) of my book, *Puritan Legacies: Paradise Lost and the New England Tradition, 1630–1890*. The argument of *Puritan Legacies* is that significant aspects of *Paradise Lost* are strikingly congruent with significant aspects of the history of New England. One of these congruences—that between the portrayal of Satan and the cultural consequences of the emer-

gence of industrial capitalism in nineteenth-century New England—Lowance ignores altogether, so I will deal only with the portion of the book he pretends to assess.

The book finds that in the central narrative of *Paradise Lost*, the Fall of Adam and Eve, Milton depicts a relationship that, in its general social definition, hovers ambiguously between hierarchy and equality. It further finds both a similar general social definition and a similar pattern of psychological nuance and vicissitude in the relations between Ebenezer Parkman, a minister of the established church, and his eighteenth-century Westborough, Massachusetts, congregation. The life of the Westborough church is claimed to be representative of colonial New England as a whole, in this particular regard, and this claim is advanced only after a range of other evidence pertinent to the issue is examined. Ebenezer Parkman's *Diary* should, the argument runs, be regarded as offering representative views of New England experience in this area, because it is consistent with the preponderance of the other available evidence, and because its greater particularity helps to disclose the full implications of that other evidence. And the affinity of this New England material with Milton's account of human origins in *Paradise Lost* thus constitutes, within *Puritan Legacies*, a description of a core component of the Puritan "structure of feeling."

What prompts me to write this letter is not that Lowance passes negative judgment on this argument but that he presumes to do so without giving his readers even the slightest hint that it exists. Instead of describing it, then pointing out its deficiencies, he merely opines that "connections are forged poorly," and he endeavors to substantiate this thumbs-down pronunciamento by ripping a haphazard selection of the book's connect-

ing assertions from their argumentative context. Such a maneuver will of course make any utterance whatsoever sound like the empty "generalizing" in which Lowance himself indulges when he mysteriously declares that the works of Jonathan Edwards would have been more representative of eighteenth-century New England Puritanism than Parkman's diary.

The reviewer's inattentiveness to the "specific evidence" and the argument that are actually presented in *Puritan Legacies* is perhaps sufficiently indicated by his puzzling reference to "the long discussions of Ebenezer Frothingham and Ebenezer Parkman." The fact of the matter is that, while the book does indeed contain a "long discussion" (two chapters) of Parkman, it devotes only a few citations to Frothingham. Readers of the *American Historical Review* would have been better served had its reviewer refrained from inventing what is not in the book and instead allowed them a glimpse of what is: an analysis, well or ill-forged, of the ways in which "the English Puritan poet and the American Puritan Commonwealth" illuminate each other.

KEITH W. F. STAVELY
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Mason I. Lowance, Jr., does not wish to reply.

THE EDITOR

ERRATUM

In John Anthony Scott's letter to the editor (*AHR*, 94 [October 1989]: 1242), the reference to the nineteenth century in the second paragraph should read "eighteenth century." We regret the error.

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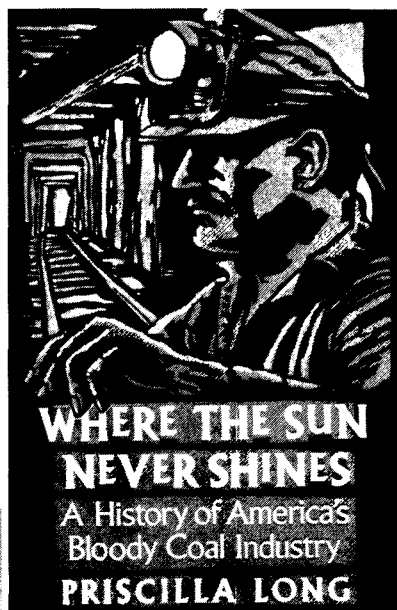
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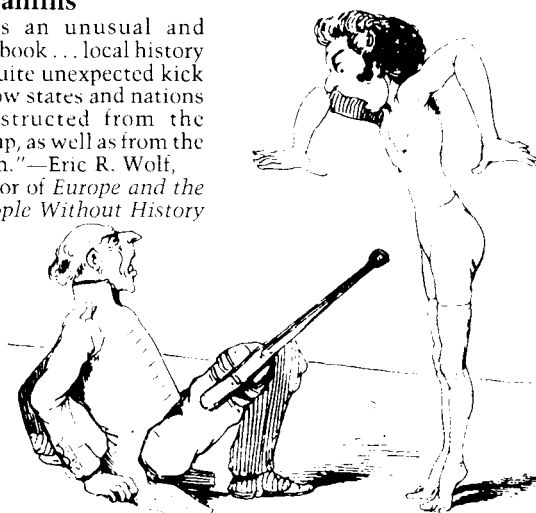
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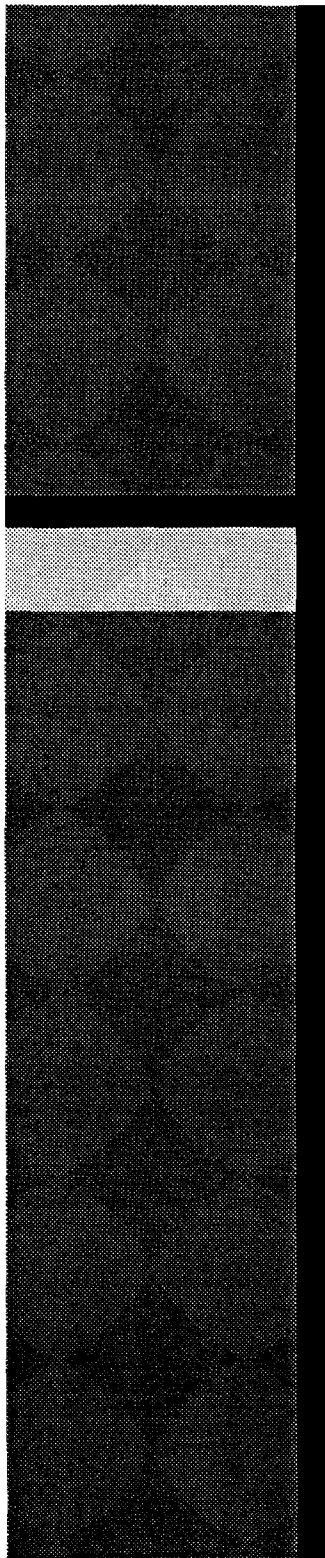
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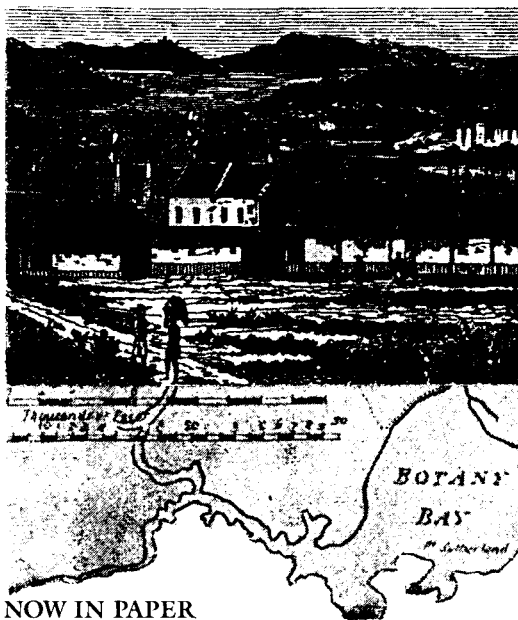
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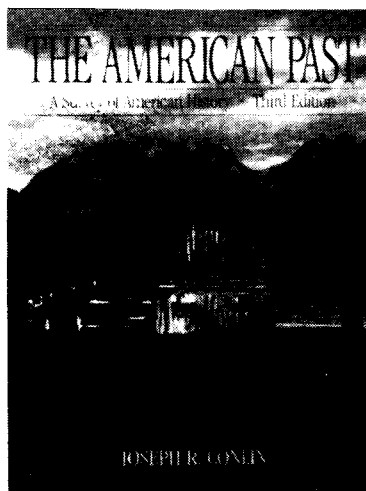
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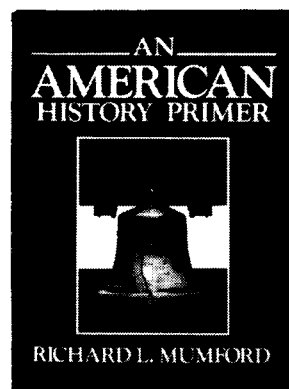
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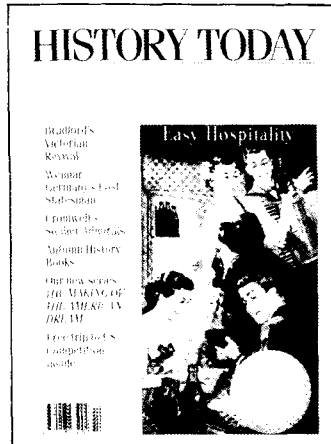
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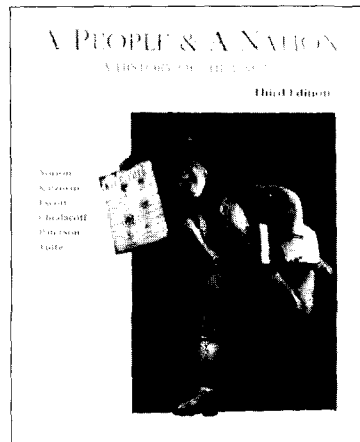
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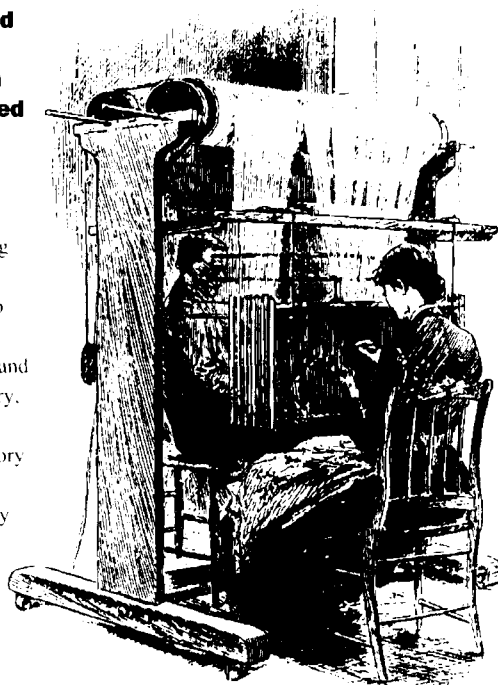
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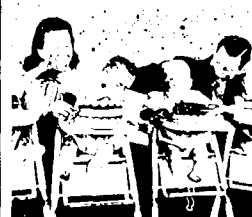
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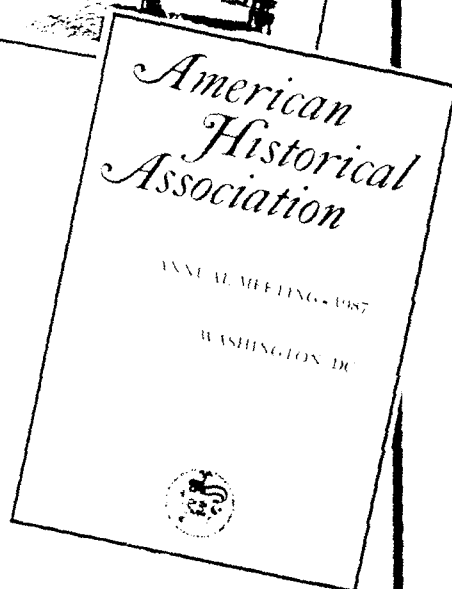
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